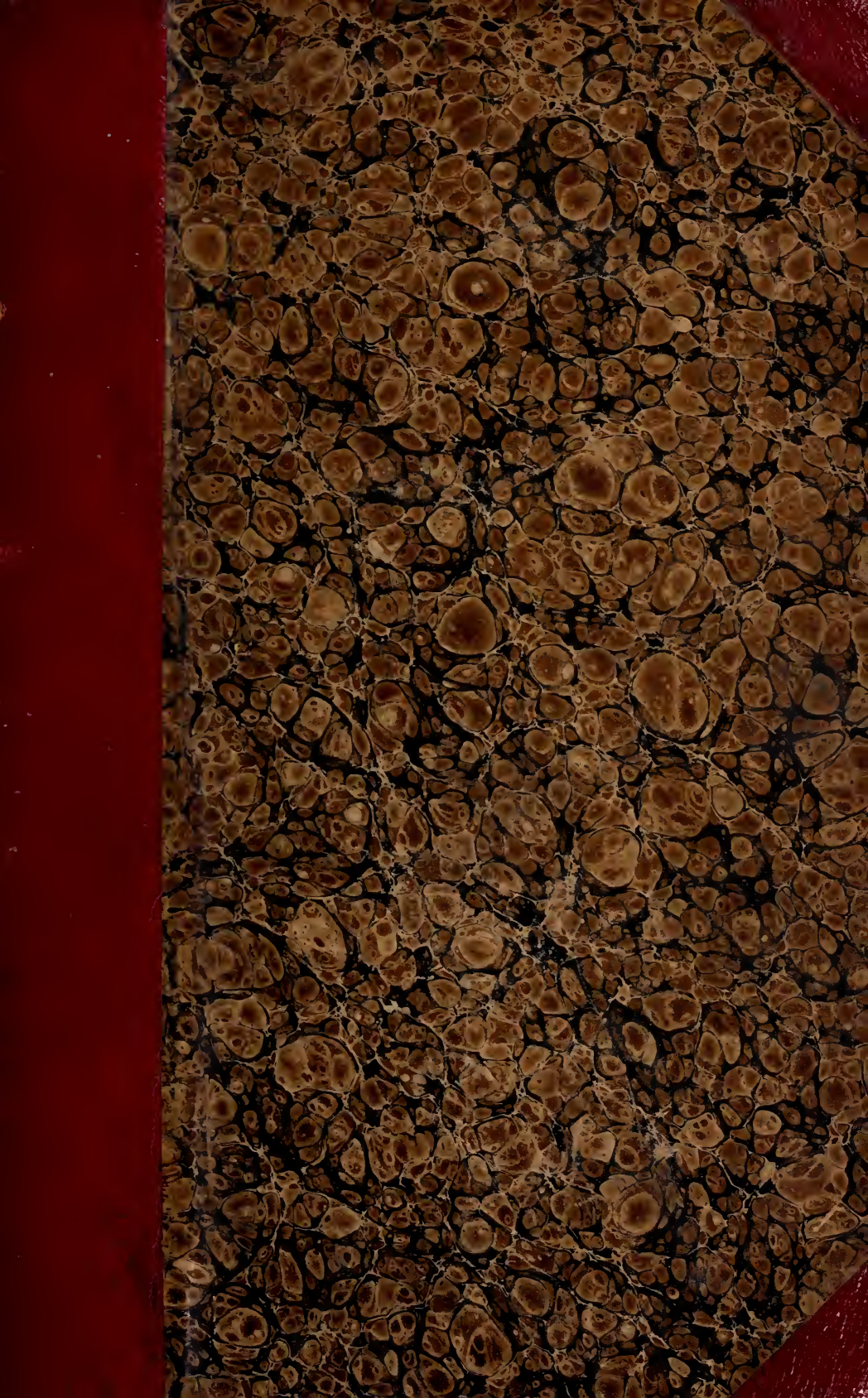
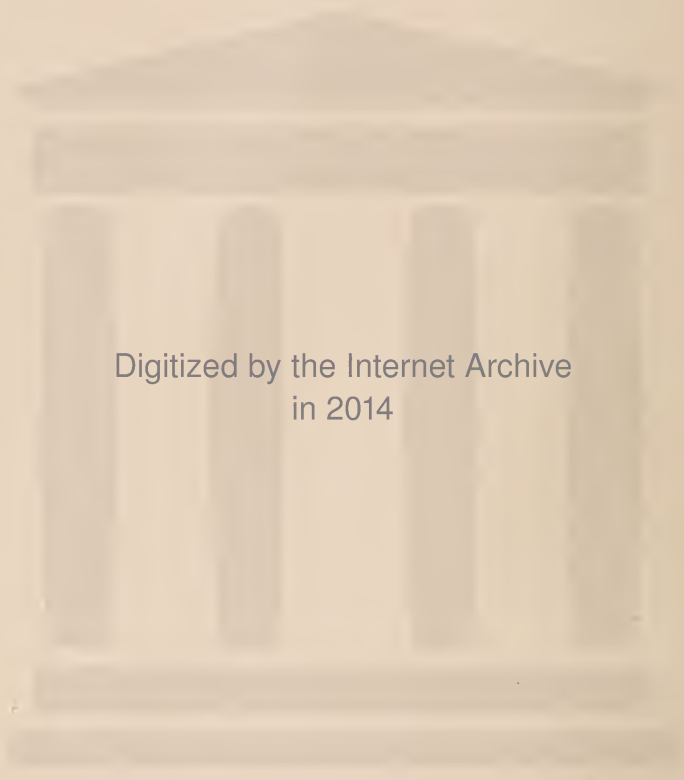




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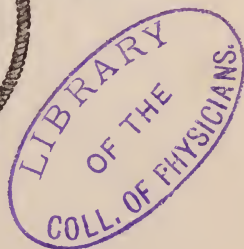
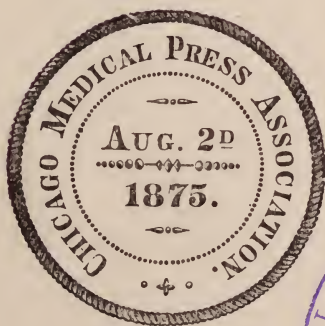
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ARTICLE I.

VACCINATION. By HENRY M. LYMAN, M.D., Professor of Physiology and of Diseases of the Nervous System, in Rush Medical College, Chicago, Ill.

It is very evident that much obscurity of thought regarding the origin and nature of vaccination still lingers in the minds of physicians. This is largely owing to the fact that a considerable period of time divides us from the epoch which was marked by the publication of Jenner's great discovery, so that many of the facts which were then widely known have now been nearly forgotten. But it is, perhaps, even more to be attributed to the circumstance that the principal authorities who have guided professional opinion were themselves ignorant of numerous facts which have been recently brought to light, and which serve to co-ordinate and to illuminate all previous observations. Hence it cannot be a matter of reproach that inadequate and erroneous opinions regarding this subject have been entertained by many fairly educated men, since even so eminent an author as Sir Thomas Watson, only a few months ago, found himself obliged, after reading Mr. Ceely's papers, which, though published forty

years previously, had never before attracted his attention, to retract his life long utterances regarding the nature of bovine virus. The recent researches of Pasteur and of his companions, regarding bacterial infection, have also contributed greatly toward the solution of the mystery which, in the minds of many, seemed to invest the whole subject of protection against small-pox by vaccination. We are now, however, in a position to form a clear idea regarding a subject, in connection with which there is no longer any place for mystery.

The opinion was long entertained vaguely by many physicians that small-pox and vaccinia were entirely distinct, though similar, diseases. This belief was based upon the fact that Jenner did not himself fully comprehend the manner in which these two forms were related. He believed that they were identical in their origin, but he did not demonstrate that identity. It was his opinion that horse-pox infected the cow through the intervention of the hands of the stable-boy who attended both animals, but he did not establish the origin of the horse-pox. It was only when the experiment of variolating the cow with the virus of small-pox had been successfully performed that the fundamental identity of variola and vaccinia was demonstrated. This was first performed, in 1801, by Gasser, of Günsberg (Seaton, *Reynolds' System of Medicine*, Vol. I., p. 176, Am. Ed.) Inoculating eleven cows with small-pox virus, he produced on one of them vesicles bearing all the character of vaccine vesicles, and from them a stock of genuine vaccine lymph was obtained. The same experiment (*loc. cit.*) was performed, in 1802, at the Veterinary College at Berlin. Mr. Ceely, of Aylesbury, in England, was equally successful in 1839. In 1840 (*loc. cit.*) Mr. Badcock, of Brighton, succeeded in procuring genuine vaccine virus by variolation of the cow; and he met with equal success on thirty-seven subsequent occasions.

It has also been shown that cattle can be variolated by inoculation of the volatile exhalations of variolous clothing, etc., but this is of rare occurrence.

Another significant fact deserves mention in this connection. Since the general adoption of vaccination, the vaccine disease has almost entirely ceased to occur in the cow. Previous to the in-

roduction of vaccination, when small-pox was one of the domestic pestilences, it was an exceedingly common thing to find cows suffering with the vaccine eruption upon the udder. It was never observed upon the bull. Nor was it discovered upon the cow, except during the period of lactation, when subjected to handling. This indicates that variola is not a primary bovine disease, but is communicated from the human sufferer to the lower animals. The identity of variola and of vaccinia (so far as their origin is concerned), and the dependence of cow-pox upon previous human small-pox may therefore be considered a demonstrated fact. Cases like that of the Beaugency heifer may be most probably explained by supposing that they originated in an infection communicated by the hand of a variolous tramp, or of some other milker who was the unsuspected bearer of the virus of small-pox.

The above mentioned experiments and observations also place in bold relief the fact that the manifestations of variola may be exceedingly varied by the conditions of their occurrence. The phenomena of inoculation for small-pox had taught this lesson long before the discovery of Jenner; and the varieties of post-vaccinal small-pox have illustrated the same thing ever since. Few diseases are more easily varied by external conditions. Thus we have discrete, confluent, and hæmorrhagic small-pox, post-vaccinal small-pox or varioloid, bovine small-pox, and vaccine disease—all springing from one common source. It is true that the identity of *variola vacciniæ* and human small-pox has been denied by Fleming and by others; but I think no unprejudiced person can read Ceely's original papers without coming to the same conclusion that was realized by Sir Thomas Watson, when he for the first time perused them, a few months ago—that they are actually one and the same disease, only modified by the different environments in which they occur. This experimental proof also corresponds with the deductions derived from observation of the protective effect of vaccination. There is no other example of immunity against any particular disease acquired by experience of some other disease. Scarlet fever protects against scarlet fever, measles against measles, typhus fever against typhus fever, small-pox against small-pox. If, then, vaccine

disease protects against small-pox, every analogy goes to show that it protects because it is, indeed, itself, only a form of small-pox. As long as we allowed ourselves to consider it a separate disease, it was impossible to refute the argument of the homœopathist, who quoted vaccination as an example of a disease being remedial against another *similar* disease. Like cures, or rather, prevents, like in this case; therefore the law *similia similibus curantur* must be true. Such was the argument, and it could not be successfully attacked by those who held to the dual nature of these diseases. But when we accept the evidence of experiment in the variolation of cows, we can reply to the homœopathist: This is no case of like and like. Small-pox protects only against itself. Variolæ vacciniæ and variola are radically and essentially the same disease, and the one must necessarily protect against the other by reason of their actual identity.

It was not, however, until the recent experiments for the protection of animals and fowls against anthrax and chicken cholera, that all difficulty was removed from consideration of the problem. Pasteur, Toussaint, and others, in France and Germany, found that by *attenuating* the virus of chicken cholera, and by heating the blood or other liquids of anthrax, a dilute form of the specific poison was produced, which, when injected into a healthy animal, excited a modified, and generally non-fatal form of the disease, by which the animal was completely protected against the fully virulent form of the contagion. Here, then, we find an explanation of the manner in which the virus of bovine variola differs from the virus of human variola—it is variolous virus attenuated, diluted or modified by the action of the bovine tissues, so that, while it does not excite the general eruption of variola in the human subject, it does yet, when used for vaccination, so sufficiently modify the human tissues that they become tolerant of the fully virulent poison of unmodified human small-pox.

It follows, therefore, that we have in the variolation of the cow an ever-present method of originating fresh supplies of vaccine virus—a method which in time of need is much more reliable than the search for accidental cases of sporadic cow-pox, both because of the danger of confounding cow-pox with vaccinella, a puerperal eruption of the cow which has no relation whatever

with small-pox, and because of the great rarity of genuine sporadic cow-pox in these days of limited prevalence of human small-pox. The discredit which some have sought to throw upon this method of producing vaccine virus grows out of ignorance of the fact that the virus yielded by the pustule of a freshly variolated cow is exceedingly energetic. It is so active that Watson and others long supposed that it was merely the original variolous matter transplanted back again from the cow to the human subject. Ceely found that it produced such acute symptoms that it was necessary to attenuate it by several successive human vaccinations before it was sufficiently moderate in its effects to be used with comfort. Such virus sometimes produces an actual eruption, closely simulating varioloid; but after a few successive vaccinations it becomes so far "humanized"—that is, *attenuated*—that it only occasionally causes such general disturbance. Sometimes, however, as I have this past winter observed, after the use of bovine virus, we get an eruption of numerous vesicles around the point of vaccination. In one instance I observed an attempt at general eruption, which, had it occurred after the use of virus from a freshly variolated cow, might have been mistaken by an ignorant person for an aborted variolous exanthem. Similar eruptions after the use of bovine virus are mentioned by all the writers on vaccination; and they would no doubt be more frequent if the original bovine virus (Beaugency, or other stock) had not been already diluted by numerous successive calf-inoculations before it reached the profession.

The superior value of bovine virus for purposes of vaccination is now fully established by the results of accurate observation in England and Europe. It was foreseen by thoughtful men very soon after the announcement of Jenner's discovery, that the protective influences of vaccination must necessarily diminish with the progress of time; but the enthusiastic advocates of vaccination would not listen to any such opinions. For a number of years, in fact, the protection against small-pox seemed to be almost perfect. But a quarter of a century had not elapsed before it was remarked that post-vaccinal small-pox was becoming frequent. It was ascertained, at length, that by multiplication of the points of vaccination a greater degree of immunity was con-

ferred. For a long time insufficiency of vaccination was claimed as the cause of post-vaccinal small-pox. But as years passed on, it was found that multiplication of marks, though better than single puncture, was becoming diminishingly protective. At the same time, the mortality of small-pox in unvaccinated persons was also gradually increasing. The following table is given by Dr. Cameron (*Fortnightly Review*, May, 1881):

Percentage of mortality in small-pox in persons showing	1836-51	1852-67	1870-79
Cicatrices of vaccination	6.9	7.6	9.2
“ 1	9.2	14.8	13.65
“ 2	6.0	8.7	10.14
“ 3	3.6	3.7	7.4
“ 4 or more	1.1	2.0	4.83
“ 1 or 2	7.9	11.5	10.29
“ 3 or more	2.4	2.8	5.8
Percentage of mortality in small-pox, or in unvaccinated persons.	37.5	35.7	44.6

During the earlier years of the century the mortality from post-vaccinal small-pox did not exceed one per cent. It has gradually increased, with temporary abatements, caused by improved hospital accommodation, treatment, etc., but on the whole, steadily increasing.

The causes of this result are two-fold: One great cause lies in the gradual loss of the protective influence of hereditary tolerance which had been acquired by an indefinitely long period of exposure to small-pox. This subject I have elsewhere discussed (*The Medical Record*, June 26, 1880, p. 720). The other causes which we at present have to consider, consists in the gradual modification of vaccine virus itself by repeated transmission to healthy subjects. In England, whence the above statistics are derived, vaccination is performed with human lymph, the greater part of which has descended directly from the original bovine virus introduced by Jenner. In other words we have here the results of a series of “culture experiments,” in which bovine

virus has been for many generations cultivated in the liquids of successive healthy human bodies. We find in these cultivations a regular progress toward a result analogous to the result of successive cultivations of the *bacillus anthracis* in healthy fluids outside of the animal. Gradually the bacillus loses its infective energy, until at length it becomes utterly innocuous, like the harmless hay-bacillus from which, by appropriate management, it was originally transformed. This process is evidently going on in the case of English vaccine virus. At its present rate of progression it is not difficult to estimate approximately the period necessary to reduce such virus to a condition of non-protective inertia. The bovine virus which we now use in this country has not yet had time to degenerate appreciably; but there is no reason to suppose that its course, whether cultivated in the body of the calf, or in the body of the human infant, will vary from the course of Jenner's original bovine virus in England.

The only sufficient remedy for this deterioration consists in occasional reversion to the original product of the variolated cow. By a few subsequent calf cultivations, this can be sufficiently attenuated for human use; and thus the original protective power of vaccination, so far as energy of the virus is concerned, may be maintained. Of course this cannot in any way compensate for that loss of hereditary tolerance of small-pox which is steadily progressing.

One more lesson we may learn from these observations. It is evident that small-pox is a disease which does not prevail among us as a direct transmission from some indefinitely remote first case of variola. It must necessarily be continually originating, *de novo*, in some part of the world. Were this not the case, small-pox, by reason of constantly progressive dilution, attenuation, or modification, of its virus by successive cultivation in the bodies of the healthy, would long since have lost its virulent character, and would have ceased to exist. We are justified in the adoption of this conclusion by what may be observed in any protracted epidemic of the disease. As Marson remarked, long ago, such epidemics cease not so much by reason of a lack of unaffected individuals in a community, as by reason of causes which he was not then in a position to explain. A sufficient ex-

planation of the fact may be found in the diminishing energy of variolous virus by reason of frequent transmission through the bodies of healthy individuals. Just as the virulent energy of the *bacillus anthracis* diminishes, and finally disappears, as a result of its cultivation in successive portions of a healthy cultivating fluid, so the variolous virus finally loses so much of its virulence during the course of an epidemic that, though capable of producing the typical phenomena of small-pox as long as it produces any effect, the newly formed virus becomes less and less capable of maintaining its specific existence, and *dies out*, as we say.

It is true that we might not be at liberty to draw this conclusion from observation of variolous epidemics alone; but such observations, taken in connection with what we now know regarding the fate of a certain definite body of variolous virus—the modified Jennerian bovine virus, studied in England during the present century—enable us to arrive at certain definite scientific conclusions of the highest importance.

I am well aware that the revivification of variolous virus which takes place whenever it is transported to a virgin soil in a susceptible population, seems to contradict the hypothesis of its deterioration by transmission from person to person. This seeming contradiction, however, is more apparent than real. If, by mere transplantation, a virus could be restored to its original degree of energy, every virulent disease should long since have become everywhere endemic within its own climatic zone; and it should now be impossible to trace the march of any epidemic. Instead of this, we can trace the course of small-pox on lines which always radiate from an oriental, endemic focus where there is every reason to suppose that the disease is continually arising, *de novo*, as a result of special local reactions between man and his environment.

ARTICLE II.

LARYNGEAL TUMORS. Report to the Illinois State Medical Society by E. FLETCHER INGALS, A.M., M.D., Lecturer on Diseases of the Chest and Physical Diagnosis, and on Laryngology, Rush Medical College; Professor of Diseases of the Throat and Chest, Woman's Medical College, Chicago, May 18, 1831.

Less than a quarter of a century has elapsed since the discovery of the laryngoscope removed the diagnosis and treatment of diseases of the throat from the field of empiricism, and placed it among the most exact specialties of our art.

No more striking illustration of the benefits that have accrued from a knowledge of laryngoscopy can be found than that which is contained in the history of laryngeal growths.

For twenty-five hundred years preceding the year 1857, not more than seventy cases had been recorded, where either ante-mortem chance or post-mortem investigation had, by the discovery of a morbid growth, revealed the true nature of many obstinate and fatal cases of laryngeal disease. Since the experiments of Czermak and Türk many of these growths have been discovered; hundreds of patients have been relieved, and many have been rescued from death by the removal of tumors which would otherwise have caused strangulation. Nearly every variety of morbid growth which may affect the human body has been discovered in the larynx, but the great majority of intra-laryngeal tumors are of a papillary character. Probably ninety-seven or ninety-eight per cent. of all these growths are benign in nature.

Of all laryngeal tumors the papillomata constitute about seventy-five per cent.; the fibromata about twelve per cent., and fibro-cellular growths about five per cent. Of the remaining, the greater part are cystic, and after these, in the order of their frequency, come the sarcomata and lipomata, together with rare instances of mucoid, vascular or adenoid growths, and finally cancers. Of primary cancer it has been my misfortune to see three cases.

Morbid growths in the larynx are found most frequently in males; they may occur at any age, but between the ages of twenty and forty, they are more common than at either extreme of life. Of my own patients, the youngest was six years old and the oldest seventy.

Excepting the malignant growths, these tumors are generally the result of chronic catarrhal inflammation of the larynx, of a mild character. They are occasionally caused by syphilis, and not infrequently by phthisis. In some instances measles, croup, diphtheria, whooping-cough, or the inhalation of irritating substances seem to have acted as exciting causes.

The symptoms caused by these growths, depend mainly upon their location and size, and are much the same regardless of the exact nature of the tumor. The patient usually gives a history of having had a severe cold, contracted several months beforehand, from which he has never fully recovered. There has generally been some hoarseness at first, which has at times been better and at times worse, until finally it has become persistent; but in some cases the aphonia continues paroxysmal for a long time. The hoarseness may progress to complete aphonia, and if the tumor is large, considerable dyspnoea may be experienced. The affection of the voice is often most marked with small tumors, especially if they are attached to the vocal cord. Often these patients complain of a tickling sensation in the throat, and when the tumor is pedunculated they frequently experience sensations like those produced by a foreign body in the larynx.

The growth seldom causes much pain, but frequently it gives rise to slight discomfort, especially on swallowing. If the tumor is of considerable size the difficulty in deglutition may be very marked. Even with small growths, speaking is often tiresome, and with the larger it may be nearly impossible, either from the impediment to the free vibration of the cords or from lack of force in the expiratory current of air.

Respiration is often stridulous when the tumor is large. Cough is usually present, but it varies greatly in character and frequency. It may be harsh and dry, or easy and loose, and it is sometimes croupy. In some cases there is scarcely any cough, while in others this may be the most distressing symptom. With

small neoplasms the expectoration is usually slight, but with the larger growths, whether benign or malignant, it is frequently excessive. In these latter cases collections in the larynx of tenacious mucus greatly add to the suffering and danger of the patient. This mucus may cause great difficulty in respiration and doubtless in cases which are not properly treated it is often the immediate cause of death.

Diagnosis.—By auscultation over the larynx or trachea a moist râle or sort of valvular murmur may sometimes be detected, but even if all the ordinary symptoms and signs of a tumor are discovered, an accurate diagnosis cannot be made without the laryngoscope.

By the aid of a small mirror, placed in the throat, and a good light reflected upon it, we can usually at once determine the nature of the difficulty; though in some cases the intractability of the patient or the peculiar location of the tumor may necessitate repeated examinations.

Prognosis.—The prognosis in cases of benign laryngeal tumors depends upon their size and location. If a tumor is small and located above the vocal cords it may give the patient no particular inconvenience, but if situated on the cord it causes more or less aphonia.

Tumors as large as a pea usually cause aphonia, even though located above the vocal cords.

The tendency with most of these growths is to gradually increase in size, though some of them, after attaining a certain size, may remain stationary for years.

When a tumor has once caused hoarseness there can be no reasonable hope for the disappearance of this symptom until the growth has been removed.

Large growths, by which I refer to tumors varying from the size of a pea to that of a filbert, often jeopardize the patient's life. They may do this by exciting and rendering permanent a harassing cough which may gradually exhaust the patient; or, in consequence of the small size of the glottis they may so interfere with respiration as to cause sudden death by choking, or more gradual dissolution through the deleterious effects of continuous imperfect aëration of the blood; or their pernicious effects may be mainly due to the difficulty which they cause in deglutition.

Malignant tumors in the larynx, so far as past experience goes, are fatal. Treatment may prolong life for a few days or months, but a time will soon come when deglutition or respiration will become impossible, and then tracheotomy or even extirpation of the larynx can add only a brief span to the patient's existence.

Treatment.—There are two plans of treating benign laryngeal tumors. First, that calculated to relieve the local hyperæmia, and, second, that for the destruction or removal of the growth.

A few laryngologists discourage all operative procedure so long as the growth does not materially interfere with the individual's means of obtaining a livelihood, or directly endanger life; and these believe that the sole treatment in many cases should be that adapted to chronic laryngitis; for example, the topical application of strong mineral astringents or mild caustics.

While this treatment is undoubtedly adapted to some cases, it should be accorded a secondary place. It is often useful as an adjunct to operative measures, but it will seldom effect a cure. I have seen tumors considerably diminish in consequence of the persistent use of such remedies, but they have soon attained their original size when the treatment was suspended, or even during its continuance. In some cases I have no doubt that such treatment stimulates the tumor to more rapid growth.

Notwithstanding the difficulties and dangers of operative procedures, I am fully in accord with those laryngologists who believe that, as a rule, benign growths in the larynx should be removed by operative measures. This should be accomplished through the natural passages when possible; and when this is impracticable, if the growth endangers life, it must be removed by tracheotomy, by thyrotomy, by supra-thyroid laryngotomy, or by division of the thyro-hyoid membrane, or by infra-thyroid laryngotomy.

However, extra-laryngeal methods should not be adopted even when endo-laryngeal methods cannot be carried out, unless the patient's life is endangered by the presence of the tumor.

Most of the papillary growths found in the larynx are not larger than a pea, but occasionally they attain the size of a walnut. They are generally multiple. These tumors are usually

attached to the vocal cords, to the ventricular bands, or to the inter-arytenoid fold. They may be pedunculated, but they are more apt to be sessile.

Most papillomatous tumors of the larynx are of a light pinkish color, and have a granular surface or are laminated like condylomatous growths. They are soft and friable, so that they may be easily crushed or pulled off with forceps. Voltolini has shown that they may occasionally be detached by frequent up-and-down movements of a sponge passed into the larynx.

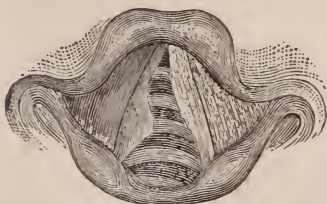
Tumors of this kind are not likely to recover after they have been thoroughly removed, excepting in phthisical patients.

As has been stated, papillomata constitute about three fourths of all laryngeal tumors, and their immediate cause is a chronic hyperæmia of the mucous membrane from which they spring.

To illustrate the earlier stages of their growth, I will cite three cases of chronic laryngitis, in which the circumscribed swelling of the mucous membrane indicates the beginning of what may ultimately become a well defined tumor.

OUT-GROWTHS.

CASE I. *Out-growths from the vocal cord.* J. K., æt. 50, laborer. This patient came to me about a year ago, complaining of hoarseness and severe cough. The lungs and heart were found to be healthy. Laryngoscopic examination revealed general diffuse inflammation of the mucous membrane of the larynx.



After a few astringent applications the patient passed from my observation. About three months ago he returned with much the same symptoms as at first. During the interim he had been sometimes near-

ly well, and sometimes quite hoarse. At this time I found the larynx greatly congested, and the vocal cords red and thickened. Upon the free edge of the right cord, about its middle, was an outgrowth of a conical form, the apex of which projected about four millimeters into the rima-glottidis (Fig. 1.) The base of this swelling extended six or eight millimeters along the free edge of the cord.

This case illustrates the origin of many laryngeal growths. At first there occurs chronic catarrhal inflammation of the part, which is followed by an excessive proliferation of cells in the sub-epithelial connective tissue, which finally results in a morbid growth supplied with new blood-vessels, and covered with an attenuated epithelium. The tumor thus formed may be single, but more often is made up of numerous papillæ, each of which may give off secondary or tertiary offsets, which give to the whole mass a strawberry-like or cauliflower appearance. The former occurs when the epithelial covering encloses the whole mass, the latter when it is insufficient to fill the spaces between the papillæ.

In cases of this kind the proper treatment consists of frequent applications to the larynx, by means of a brush, of some strong astringent, such for example, as: *Argenti nitras*, gr. xl—lx *ad aquam* ʒi; *zinci sulphas*, gr. xxx—lx *ad aquam* ʒi; *zinci chloridum*, gr. xx—xxx *ad* ʒi, or *liq. ferri per chlorid.*, min. xv—xxx *ad aquam* ʒi. At the same time sedative inhalations or internal remedies may be needed to relieve cough. In addition to these, benefit will sometimes be derived from stimulating inhalations which may be employed by the patient at his home.

In the case of this patient I have obtained the best results from applications of sulphate of zinc, gr. xxx *ad* ʒi, and the internal use of bromide of potassium with small doses of belladonna. Inhalations could not be employed. His visits have been irregular and the treatment has been correspondingly unsatisfactory; but the congestion has been much relieved, the prominence on the vocal cord has been considerably reduced in size, and the voice is correspondingly improved.

Case II. *Incipient laryngeal tumor.* J. S., æt. 43, printer.

When this patient first consulted me he had been hoarse for six months. He had been troubled most of the time with cough, but his general health had been good until a few weeks before I saw him.

Inspection of the larynx revealed swelling of the mucous membrane just beneath the right vocal cord at its posterior extremity (Fig. 2.) The prominence extended along the side of the larynx

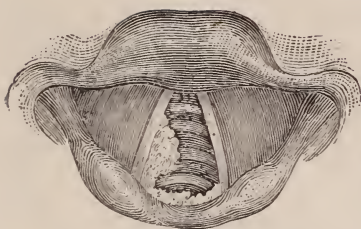


Fig. 3.—Incipient laryngeal tumor.

which was visible.

Treatment.—Astringent applications were made with the laryngeal brush and by the insufflator, and tonics were given internally.

The patient was soon obliged to discontinue his attendance at my office on account of his work, so that the effects of treatment could not be demonstrated.

CASE III. *Subglottic œdema, resulting from chronic laryngitis, and having the appearance of incipient tumors.* W. T., æt. 65, farmer. The patient stated that he first had trouble with his throat twenty-nine years ago, as the result of measles. At that time hoarseness lasted two years. About eight years later hoarseness again returned and continued troublesome for several years. He has had repeated attacks of eczema for twenty years, and for the past eighteen months has suffered from it constantly. When the patient consulted me he was very hoarse and complained of some dyspnoea on exertion, and of constant though slight sore throat.

Upon laryngoscopic examination I found the right ventricular band so much swollen that it nearly hid the right vocal cord. The mucous membrane below the anterior extremity of the left cord, and that below the posterior extremity of the right cord were swollen so as to present the appearance of morbid growths, and to considerably interfere with respiration. The condition of the throat was apparently due to the same cause as the inflammation of the skin.

PAPILLARY GROWTHS.

CASE IV. *Papilloma of larynx.* P. S., æt. 28, butcher. This patient stated that about three years before consulting me he had several attacks of sore throat which left him with hoarse-

for nearly a centimeter, and stood out beyond the vocal cord about four millimeters. The mucous membrane covering the laryngeal side of the inter-arytenoid fold presented a notched and yellowish gray appearance, strongly suggestive of ulceration below the part

ness which had been constantly present for two years. I could obtain no history of either phthisis or syphilis. The lungs and heart were normal.

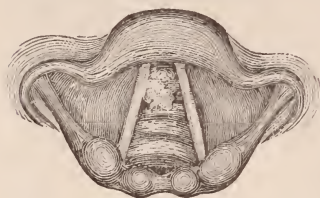


Fig. 3. Papilloma on right vocal cord.

Laryngoscopic examination revealed a tumor about the size of a large pea, on the right vocal cord near its anterior extremity. The tumor was of a light red color; it had a slightly granular surface and it was attached to the free edge of

the cord by a large pedicle.

At the first sitting I removed, with common laryngeal forceps, about half of the growth, and at the second sitting, three days later, I removed all that remained and cauterized the base with the solid nitrate of silver. To prevent inflammation I directed the application to the neck of hot applications, which were to be continued as long as there was any soreness of the larynx. At the patient's next visit, three days later, the vocal cords were still congested. Soreness of the larynx had lasted but a few hours after the operation. At this time I applied to the cords a stimulating solution of chloride of zinc gr. v. *ad.* aquam $\mathfrak{z}$ i, and ordered bitter tonics to correct some gastric disturbance.

The patient was not seen again for a month; at that time the voice was perfect in ordinary conversation, but not reliable for singing. The vocal cords were still of a pink color. I then applied a solution of sulphate of zinc, gr. xx. *ad.* aquam $\mathfrak{z}$ i, and ordered the daily use of a spray from a four-grain solution of the same remedy. About six weeks later the patient came again to my office, when I found the vocal cords of a natural color and the cure complete.

CASE V. *Tumor of the left ventricular band.* J. S. M., æt. 28, physician. When this gentleman consulted me he had been troubled with hoarseness for several months. An examination revealed chronic inflammation of the pharynx with a highly congested state of the larynx.

The left vocal cord was nearly hidden from view by the swollen ventricular band, and on the posterior half of the same band I discovered a sessile growth of a light color, ovoid in form,

about six millimeters long, and projecting about three millimeters from the surface.

The patient's throat was too irritable for immediate operation, and as he was obliged to leave the city the same evening, nothing was done. I have not seen him since, but he informs me by letter, that his throat seems in the same condition, and that he has done nothing for it.

CASE VI. *Papilloma of larynx*. Mrs. B. S., æt. about 25. Ten years ago this patient was nearly suffocated in consequence of laryngeal tumors. These were removed by Prof. Wm. Bruns, of Tübingen, and according to the patient's statement, the larynx was thoroughly cauterized with nitrate of silver and the galvano-caustic. Afterward the patient had no more trouble until a few months since, when she began to complain of a sensation as of a bolus in the throat.

Upon examination of the larynx, I discovered a small, semi-transparent growth at the anterior end of the right vocal cord. This was about four millimeters in length, by two or three in breadth, and it protruded from the upper surface of the cord about two millimeters. A papillary growth was also found on the inner side of the upper part of the left arytenoid cartilage.

The smaller growth I destroyed by the solid nitrate of silver. The larger I removed with forceps, and then thoroughly cauterized its base with solid nitrate of silver.

The removal of the tumor, in this case as in most others, caused little pain; but the caustic was very painful. Hot fomentations were directed to be kept constantly applied to the neck, for from twenty-four to thirty-six hours, or until all soreness had disappeared. In this patient the papillæ at the base of the tongue were much enlarged, and they doubtless had something to do with the sensation of a foreign substance in the throat. This case illustrates the occasional tendency of papillomatous growths to repullulation.

CASE VII. *Papilloma of larynx*. A. J., æt. 39, machinist. This patient stated that he had been more or less hoarse for three months before coming to see me. He had never had syphilis and had no knowledge of a hereditary predisposition to consumption. He occasionally had slight pain on swallowing

and he sometimes suffered from choking spells. I found the patient's digestive organs in good condition; he was well nourished and had no fever. His lungs and heart were healthy. Upon examining the larynx I found the epiglottis congested, but not swollen; the arytenoids slightly swollen, and the vocal cords slightly congested. A large papillary growth about seven millimeters in breadth by five millimeters in altitude, and one and one-half centimeters in length, occupied the laryngeal side of the inter-arytenoid fold and hung between the cords.

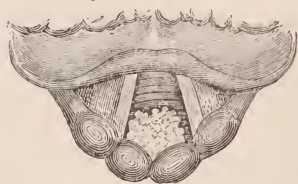


Fig. 4. Papilloma on inter-arytenoid fold.

This tumor though as large as an ordinary raisin, was completely hidden during phonation, and owing to the pendant position of the epiglottis it could seldom be seen at other times. Owing to difficulty in getting this patient to inspire easily and thus expose the growth, I thought it advisable not to attempt its removal until by topical applications the congestion of the parts had been partially relieved and the larynx had become tolerant of instruments.

Only a few applications were made to the larynx when the patient discontinued his visits to my office, but he has lately returned, and I have on two occasions introduced forceps into the larynx, but I have not yet been able to reach the growth. Although this tumor hangs very low in the larynx, I expect to seize it as soon as manipulation renders the patient's throat a little more tolerant.

CASE VIII. *Papilloma on under surface of epiglottis.* E. C., æt. 24; brick manufacturer. Two years before calling on me this patient had pneumonia in the upper lobe of the right lung, and he had been subject to frequent attacks of intermittent fever. Three months before consulting me he had an attack of pleurisy from which he recovered, but ever since he had been troubled with sore throat and hoarseness. The latter had been most annoying in the morning. He had been very much troubled at night with cough though there has been only a small amount of mucus expectorated. When I first saw him he had slight pain at intervals on deglutition, and, in consequence of

this pain and broken rest, had lost six or seven pounds during the last twelve weeks. I found that he had never suffered from specific disease, and none of his relatives so far as known, had died of consumption. The apex of the right lung showed slight evidence of consolidation, but there was no dullness on percussion.

Upon examining the larynx, I found an omega-like epiglottis, swollen to twice its normal thickness, with a small spherical papillary growth about six millimeters in diameter on its under surface near the vocal cords. The ary-epiglottic folds were thickened and slightly pyriform in shape, and the whole inner surface of the lower half of the epiglottis, of the anterior end of the ventricular bands, and of portions of the true cords, had a granular appearance with here and there erosion of the mucous membrane. From the respiratory sounds and the appearance of the larynx, I made the diagnosis of laryngeal phthisis.

Treatment: The larynx was penciled with a solution of morphia, gr. iv, carbolic acid, gr. xxv, and tannic acid, gr. xxx, in equal parts of glycerine and water sufficient to make one ounce. This had the effect of relieving pain, of giving immunity from cough at night and thus securing rest, and of somewhat reducing the swelling of the larynx. The papilloma was removed with forceps and the larynx was touched three or four times with solid nitrate of silver.

At the end of the first week the patient was directed to inhale the compound tincture of benzoin, morning and evening (℥i, to aq. Oi, at 150° F.) He was subsequently given cod liver oil. The local applications to the larynx were continued as at first with the substitution now and then of a solution of chloride of zinc, gr. xv to ℥i of water. I find the following notes in my case-book after two weeks of treatment: pulse 104, temperature 100° F.; appetite fair and cough moderate; patient has gained three pounds in flesh.

At this time he returned to his home in Indiana, with the advice that he continue the inhalations and cod liver oil, and as soon as practicable make a change of climate. I have recently learned from my friend Dr. Cassidy, of South Bend, Indiana, that this patient shortly after returning to his home went to Austin,

Texas, where he died quite suddenly about four months later. The immediate cause of death could not be ascertained.

CASE IX. *Tumor on right vocal cord.* R. D., æt. six years. I learned that this little patient had been apparently perfectly healthy since he was two months old, but three months before he was brought to me his voice began to fail, and at the time I saw him there was complete aphonia, the patient being unable to talk except in a whisper. After considerable difficulty, arising from the intractability of the patient, and a pendent epiglottitis, I obtained a view of the larynx, which revealed congestion of the cords and a small sessile tumor near the middle of the right vocal cord on its free edge. (Fig. 5.) There appeared to be some growth below the cord but it was impossible to make a satisfactory examination. As there was no dyspnœa an operation did not seem advisable.



Fig. 5. Tumor on right vocal cord.

In cases like this, operative procedures are not advisable unless, from increase in the size of the growth, dyspnœa should become marked, then tracheotomy should be performed. Then, if the tumor cannot be removed through the mouth, thyrotomy is likely to become necessary. Internal remedies may be of service in lessening the congestion of the mucous membranes and in retarding the growth of the tumor, and astringent or slightly stimulating inhalations should be employed for the same purpose.

FIBROMATA.

Fibrous tumors of the larynx are usually of small size and are generally located on the vocal cords; sometimes, however, they attain the size of a cherry. They may be attached to other parts of the larynx.

These growths are usually rounded in outline, single, and pedunculated, but they may be nodular as though made up of several tumors bound together by an investing membrane, and they are sometimes sessile.

They are usually of a grayish white or red color. They grow slowly, and when once removed have no tendency to recur. One of these tumors, which I treated, was developed under a

own observation, in a young lady affected with slight catarrhal laryngitis. The history of the case is as follows:

CASE X. *Fibrous tumor of left vocal cord.* Miss D. H., æt. 22. This patient came to me complaining of hoarseness, which, as determined by a laryngoscopic examination, was caused by simple catarrhal inflammation of the larynx. Her general health was good, and after being treated a few days, her voice was so much improved that she discontinued her visits and did not return for several months. This time I found a small fibrous growth attached to the free edge of the vocal cord near its anterior extremity. (Fig. 6.)

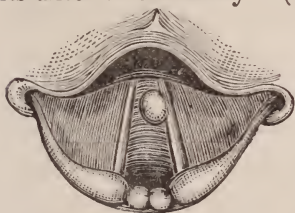


Fig. 6. Fibroma of left vocal cord.

Astringent applications were made to the larynx from time to time for several weeks, with the effect of relieving the inflammation of the parts surrounding the growth, and at times apparently reducing the size of the tumor itself; but notwithstanding the apparent improvement, the tumor grew until it reached the size of a small pea. I then attempted its removal by means of Mackenzie's tube forceps. The throat was very sensitive at first, and it was not until after numerous sittings that I succeeded in grasping the tumor, and then, owing to its firmness, only a part of its mucous covering was removed. I immediately seized the growth again and crushed it as thoroughly as possible between the blades of the forceps. The patient was then sent home with instructions to guard against taking cold, and to return in four days. The operation caused but little pain.

On her next visit, there was still some swelling of the vocal cord, but the tumor had disappeared. There was still hoarseness, but the voice was improved. The patient experienced some pain in swallowing, the evening after the operation, but this disappeared in a few hours.

When I saw her again, two months after the operation, all swelling of the cord had disappeared, and she informed me that the voice had been perfect for several weeks.

CASE XI. *Tumor of the larynx apparently fibroid.* Mr. J. M., æt. 63. Upon consulting me this patient stated that his

voice had been weak for six or seven years; he had been troubled more or less with hoarseness for four or five years and for the past three years he had been constantly hoarse. For three months he had been troubled with dyspnœa, and for the last three weeks all the symptoms had been aggravated. The difficulty in speaking seemed mainly due to deficient force of the expiratory current of air. On laryngoscopic examination a large pyriform growth was found immediately beneath the vocal cords; against which it was pressed in phonation. (Fig. 7.)

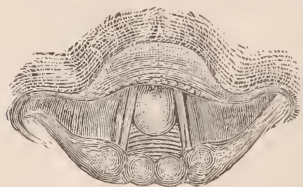


Fig. 7. Sub-glottic tumor of larynx.

The tumor was of a pale-pink color, was about eight millimeters in diameter, and was attached to the anterior surface of the larynx by a large pedicle. An operation was recommended but the patient declined to have the growth interfered with, stating that

he had lived with it for several years and thought he could do so several years more.

FIBRO-CELLULAR TUMORS.

Fibro-cellular tumors are sometimes classed as soft fibromata. They are rare. They grow slowly, but may attain a large size. When removed they have no tendency to recur. They consist of delicate fibro-cellular structure, the interstices of which are filled with fluid or semi-solid granular matter containing nucleated cells. One small growth of this character has fallen under my observation, and I have treated another which seemed to me of the same nature, though I could not be certain in the diagnosis. In both cases, the notes of which are given below, I removed the tumor.

CASE XII. *Small fibro-cellular growth on anterior extremity of right vocal cord.* Mrs. L. S., æt. 20. This patient came to me complaining of slight hoarseness and inability to use the voice for more than a few minutes at a time. She also noticed, occasionally, a slight dull pain in the throat. The difficulty dated from a slight cold fifteen months previously.

Upon examination of the larynx I found the epiglottis hanging so far back as to render it difficult to expose the anterior extrem-

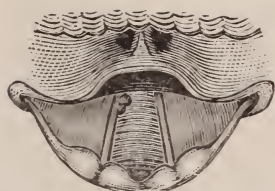


Fig. 8. Tumor on right vocal cord.

small nodules. (Fig. 8.)

The growth was removed with Mackenzie's tube forceps. Three days later no trace of it remained excepting slight congestion of the vocal cords; and in ten or twelve days the cure was complete, the voice having regained its normal condition. Over two years have now elapsed without any return of the laryngeal trouble.

CASE XIII. *Small sessile fibro-cellular tumor on left vocal cord.* Mr. E. B., aged 30. When I was first consulted, this patient had been slightly affected with nasal catarrh for about three years, and had been troubled with hoarseness for several months. I

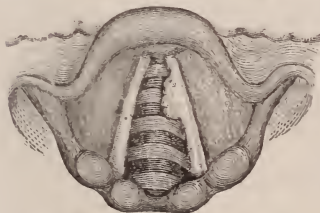


Fig. 9. Tumor on left vocal cord.

found a sessile tumor on the lower surface and free edge of the left vocal cord, about two millimeters from its anterior extremity. (Fig. 9.) The growth was about five millimeters in length, and projected about two millimeters beyond the free margin of

the cord. The mucous membrane of the post-nasal space and of the pharynx was considerably relaxed.

This patient's throat was so sensitive that several sittings were necessary before instruments could be tolerated. Astringents were applied at each sitting, and finally the tumor was removed with Mackenzie's common laryngeal forceps. Ten days later no trace of the growth could be seen, and in about six weeks the voice was perfectly natural.

CYSTIC TUMORS.

Cysts are seldom found in the larynx. Those which have been observed have generally sprung from the epiglottis or from one of the ventricles. They may attain a large size.

Contrary to what would be expected from our knowledge of other retention cysts, these tumors are not likely to return if they are thoroughly laid open, their contents emptied out, and the cavity cauterized with solid nitrate of silver.

CASE XIV. *Cystic tumor.* Mr. B., æt. 20. The only growth of this kind with which I have met, occurred in the larynx of a young farmer who could not remain in the city long enough for operative procedures. The patient had been hoarse for several months but was not troubled with cough or dyspnœa. A laryngoscopic examination revealed a small cystic growth, about eight millimeters in diameter at the posterior extremity of the right ventricular band.

The patient's throat was too sensitive for immediate operation, and, as he was obliged to return to the country on the following day, no effort was made to open the cyst.

CANCER OF THE LARYNX.

In the early stages of cancer of the larynx the diagnosis is often doubtful, as at this time the symptoms due to the laryngeal tumor are not usually different from those caused by benign growths, and often the essential cachexia does not present itself. Pain, dyspnœa and dysphagia are often present, but these symptoms vary greatly in different cases according to the seat, size, and condition of the growth. Fauvel states that at first the pain is confined to the larynx and that it does not radiate to the ears until ulceration takes place.

In one of my cases the acute, burning pain commencing in the larynx, radiating to the upper part of the pharynx, and later, to the right ear and right side of the head, together with the presence of a large nodular tumor involving the ventricular band, ary-epiglottic fold and epiglottis, left no doubt as to the nature of the case. But, in the other cases, nothing either in the symptoms and general condition of the patient, or in the physical appearance of the growth enabled me to make an exact diagnosis. In these cases at first the physical appearance of the growth was so much like that of an ordinary papilloma that only a microscopical examination by an experienced pathologist could determine its true nature. Even then the fact that many tumors of the larynx which have had a malignant appearance to the mi-

croscopist, have had a benign history and course, induced me to hope that these might possibly be of a benign character; however, in both instances the subsequent history has justified our worst misgivings.

For cases of laryngeal cancer there can be but one prognosis, as thus far all that have been reported, with three exceptions one of which was probably non-malignant at first, have died within a few months after the disease became developed sufficiently to cause the patient to seek advice. Mackenzie states that the usual duration of epithelioma of the larynx is about eighteen months, and of encephaloid three years.

The forms of treatment which must be considered in cases of laryngeal cancer are: removal through the natural passages and thorough cauterization of the base of the growth; tracheotomy; thyrotomy; and extirpation of the larynx.

In cases where there is a well defined tumor, of doubtful character the first of these methods seems preferable; but when the growth is not well defined and when it springs from the sub-mucous tissues, or where it involves a considerable portion of the larynx, this method cannot effect its complete removal, therefore, one of the other methods must be tried if anything is done.

Tracheotomy will usually add several months to the patient's life, and has prolonged it in some cases for two years.

Thyrotomy.—The results of thyrotomy with removal of the growth have been very unsatisfactory. In some cases where this operation has been attempted, it could not be completed; in others the patients have died in a few days, and in nearly all of the remaining cases the growth has speedily returned.

Extirpation of the larynx may be practiced in suitable cases, but it is an operation attended with great danger, in which, as stated by Dr. P. Kock, "The skill of the surgeon is, in some cases, shown by the patient not dying under his knife." I find records of twenty cases in which this operation has been performed. Of these, eight died in from two to fourteen days; one in six weeks, and in eight the disease returned and proved fatal in a few months. Of the three remaining cases one died of phthisis a year and a half after the operation; and in the other two there has been no return of the affection.

All things considered, when endo-laryngeal treatment cannot be successful, tracheotomy seems to hold out the greatest encouragement to both surgeon and patient; but even this in most cases should not be strongly urged by the surgeon as at best it can only add a few months to a miserable existence. One of the arguments used in favor of the removal of cancers which can be easily reached is, that when they return they may affect some vital organ and thus terminate the patient's life without prolonged suffering. If this argument was applied to cases of cancer of the larynx even tracheotomy would never be advised.

CASE XV. *Cancer of larynx.* S. P., æt. 69. This patient came to me complaining of hoarseness and great dyspnœa. The former had lasted eighteen months, the latter had been present six weeks.

Upon examination a large growth was found on the right side of the larynx apparently springing from the ventricular band, and extending along its whole length. About three-fourths of the glottis was obstructed by this tumor. (Fig. 10.)

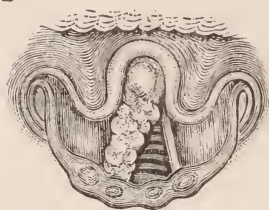


Fig. 10. Cancer of larynx.

Owing to an omega-like epiglottis and to difficulty in getting the tongue sufficiently out of the mouth, I had great difficulty in obtaining a good view of the growth, and subsequently I found even greater difficulty in introducing forceps for its removal.

At one of the first sittings a portion, the size of a large pea, was removed and submitted to Prof. Danforth for microscopic examination. He pronounced it a semi-malignant growth likely to return. Several sittings were necessary before the tumor was entirely removed. At the end of six weeks my notes state that although the growth had been removed the patient complained of great weakness; for which he was given tonics. Three weeks later I was called to see him at his home. He was greatly prostrated, and, on examination of the larynx, I found a swelling about the size of a filbert, which had the appearance of an abscess of the right ventricular band. This was incised freely with laryngeal lancet two or three times with the effect of materially reducing its size though no pus escaped. Subsequently fungoid

granulations sprang up and grew so rapidly that in two or three days they nearly stopped the glottis. These were removed every second or third day for about two weeks. At that time the sub-mucous tissues became involved more extensively and the obstruction of the larynx could not longer be relieved by forceps.

After consultation with Drs. H. A. Johnson and E. Ingals, it was decided that no considerable relief could be afforded without tracheotomy.

The case was fully explained to the patient but he hesitated about the operation, and death terminated his sufferings about thirty-six hours later.*

CASE XVI. *Cancer of larynx*. P. N., æt. 59, laborer. Four months before consulting me, this patient began to have slight burning pains in the throat, on deglutition. Shortly afterward these pains frequently occurred at other times, and finally they became nearly continuous, with frequent exacerbations. The pain began in the lower part of the larynx from which it would dart to the upper part of the pharynx, and finally to the right ear and the right side of the head. Hoarseness had been present for nearly four months. At this time the patient's appetite was fair and the general health seemed as good as usual. He did not suffer from dyspnœa when quiet excepting while lying down.

An examination of the larynx revealed a large nodular growth about two centimeters in diameter which involved the right ventricular band, the right arytenoid cartilage and ary-epiglottic fold and about one-fourth of the right side of the epiglottis. (Fig. 11.) The tumor hid the posterior four fifths of the glottis

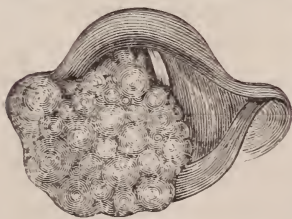


Fig. 11. Cancer of larynx.

from view, but the anterior extremities of the vocal cords appeared healthy. The diagnosis of cancer was made and an unfavorable prognosis given; which so discouraged the patient that he sought more favorable opinions from other physicians. Two months later

I was called to see this patient at his home. The growth had then ulcerated and great destruction of

* This case was at first supposed to be non-malignant, and was reported as such to the Chicago Medical Society.

the upper parts of the larynx had occurred. The patient was extremely weak, was suffering greatly, and was unable to eat. He had failed rapidly in the last two weeks. Anodynes were given and the friends informed that the end was near.

I have been unable to learn the subsequent history of this case.

CASE XVII. *Cancer of larynx.* R. S., æt. 53, farmer. Tumor on left ventricular band. When this patient first consulted me last September I found that he had been hoarse for eighteen months, and had suffered considerably from cough, which occasionally caused pain. There was no dyspnœa and no difficulty in deglutition.

Upon examining the larynx I found a large whitish growth with a granular surface, which extended the whole length of the left ventricular band from which it seemed to spring. The tumor projected from the surface nearly a centimeter. It hid the left vocal cord completely and covered about one-third of the glottis. An attempt to remove the growth with the ordinary laryngeal forceps failed on account of its firmness, but two or three pieces as large as peas were removed and a microscopic examination of these by Prof. Danforth, led to the conclusion that the growth was of a semi-malignant character. The patient was obliged to return to the country before any further operative procedures could be instituted. He was then given iodide of potassium freely for several months.

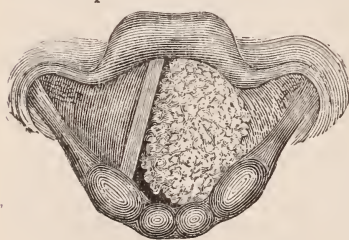


Fig. 12. Cancer of larynx.

was much increased.

By means of laryngeal knives and forceps, I removed at the first sitting, about two-thirds of that portion of the growth which was visible.

Two days later the patient returned, and I then discovered that the growth extended downward some distance below the

This patient returned to the city last week. Since I saw him in September, 1880, the tumor had grown so as nearly to obstruct the view of the glottis. Only the right vocal cord and a small part of the rima glottidis, about two millimeters in width, could be seen. (Fig. 12.) Dyspnœa had become quite constant, and the aphonia

vocal cords. With the knives and forceps I now removed a large part of the obstructing mass so as to leave a free opening for respiration.

Prof. I. N. Danforth has examined portions of the tumor removed at the first sitting. He states that the growth has passed the semi-malignant period and that it is now a true cancer.

Prof. Bridge has examined some of the pieces removed at the second sitting and corroborates this serious diagnosis.

P. S.—This growth was removed as completely as possible, with laryngeal knives and forceps, and its base was thoroughly cauterized with solid nitrate of silver.

When he returned to his home the respiration was easy, the glottis being about three-fourths its normal size. His physician, Dr. H. Reineking, was requested to open the trachea as soon as dyspnœa again becomes urgent.

P. S.—Just as this was going to press I received a letter from Dr. R., stating that a few days after the patient's return, he found the sub-glottic portion of the growth somewhat enlarged since the operation. Twelve days later the enlargement was so great as to considerably obstruct respiration, and on the twelfth of June, about three weeks after the operation, he had been called in the night to see the patient, who was having great difficulty in respiration. Perceiving that tracheotomy would be necessary, he sent for another physician to assist him in the operation, but the messenger had hardly left the house when the patient ceased breathing.

The doctor promptly opened the trachea with no assistance save that of a few laborers, breathing was restored, and now the patient bids fair to make a good recovery from the tracheotomy.

ARTICLE III.

TREATMENT OF PNEUMONITIS, WITH ILLUSTRATIVE CASES. By
P. O'CONNELL, M.D., Sioux City, Iowa.

During the past four years the treatment of pneumonitis has been frequently discussed in current medical literature. Several able articles have appeared, from time to time, in the journals, advo-

cating and detailing different and equally successful plans of treatment. The hope that others may try a method which is simple, inexpensive, and easily carried out, with the same gratifying result that I have had from it, prompts me to forward this contribution to the literature of the subject. Possibly the good result attained under very simple treatment may be a coincidence—may be only the natural history of pneumonia and not the result of the treatment employed. Although being *post hoc*, I consider it fairly *propter hoc*. Acute idiopathic lobar (“croupous?”) pneumonitis is referred to in this connection.

The *antipyretic treatment*, in contradistinction to that usually followed by most practitioners in hospital and in private practice, consists essentially of the cold bath, large doses of quinine, and salicylic acid.

The *cold bath* is a very powerful means of abstracting heat from the body. The patient is kept in the bath during ten to twenty minutes or more, or until the temperature is reduced to about 100° F. The bath is always repeated under the personal supervision of the medical attendant, as often as the temperature mounts up to 103° F., or over. It requires frequent repetition, the reaction being often great and sudden, and the effect, therefore, but temporary. It certainly is difficult and troublesome to carry out, and is not seldom attended with some risks and unpleasant consequences. To patients and friends this plan seems not only heroic, which it really is, but positively appalling, and is consented to reluctantly. After fair trial it has not yielded very good results, and now has few advocates except among hospital physicians among English speaking peoples. For private practice it is practically useless.

Quinine, in some form, has many more advocates, and deservedly so. It is given by some in grain doses every hour, or five grains every four hours, or ten grains three times a day. Others give twenty to thirty grains morning and night, while a few give forty to fifty grains at once, and do not repeat the dose for twenty-four hours.

At Bellevue Hospital, New York, the cold bath was tried. The result was disappointing, and it was soon given up. Dr. James, of Frankfort, Kentucky, after a fair trial of the cold bath

does not think very highly of it. In his opinion the result was not proportionate to the disturbance to the patient, nor to the risks, nor to the labor necessary to carry it out. With it the deaths amounted to seventeen per cent.; with quinine, in large doses, the death rate was twenty per cent.; salicylic acid proved a total failure in his hands. It is but just to say that the above figures were obtained by Dr. James during an outbreak of sewer gas pneumonia, when the mortality will necessarily be high under any and all forms of treatment.

Antimony, for its expectorant and diaphoretic effects, will be beneficial in some cases. Its sedative action on the heart proves very useful occasionally. In strong, vigorous patients I have found it answer well. Children under five years of age cannot, of course, safely take it except in rare cases.

Opium, especially when there is much pain, will be both useful and necessary. Diaphoretics and expectorants, to which may be added small doses of opium and sometimes antimonial wine in nauseating or non-nauseating doses, as occasion may require, will often prove a useful combination. Yet, out of a total of sixty-four cases treated by Dr. Thomas Barr, of Glasgow, Scotland, with antimony, opium, diaphoretics and expectorants, or with a combination of these agents, he had a mortality of *one in six* in private practice.

There is quite a unanimity of opinion as to the benefit of external applications to the chest, over the inflamed lobe or lobes, linseed poultices being in greatest favor. Moist warmth is both soothing and agreeable to the patient. Frequently it is all that is necessary to relieve the stitch-like pain. In my opinion it favors and hastens resolution. Mustard, turpentine, and even blisters, occasionally, may be required, but not until the consolidation of the pulmonary parenchyma tends to linger.

The treatment which I now practice is as follows: A piece of thick white flannel, loosely wrung out of hot water, is wrapped round the chest and covered with some material impervious to air and moisture, such as oiled silk, gutta percha tissue, or thin oil cloth. If only the lower lobe in one or in both lungs be inflamed, the flannel need not extend higher than the axillæ. But when an entire lung is involved, then the flannel must cover

the entire thorax, apertures for the arms to pass through being cut in it. I deem it essential, and, therefore, always insist, that the flannel shall extend completely round the chest and overlap a little, at the ends, on the front of the thorax, whatever may be the extent of lung tissue involved. The outer air-tight covering should be a little larger, every way, than the flannel, so that the heat may not escape under the upper nor lower edge, nor at the sternum where the ends overlap. In all this there is nothing new. Dr. Flint speaks of it in his Practice of Medicine. Flannel heated and covered in this way retains its warmth quite as long as a poultice. I have, occasionally, used linseed poultices, but give the preference to the hot moist flannel, because it is much more cleanly and less troublesome; it is easily renewed by dipping it again in hot water and loosely wringing it. Besides, few can be relied upon to properly and efficiently make a linseed poultice.

Then genuine* James' Fever Powder (*Pulvis Jacobi Verus*), one to five grains every two, three, or four hours, according to age, is prescribed. Five grains, to adults, is the maximum dose employed by me, while one grain can be given to a child under six months. The warmth of the flannel, aided by the James' Powder, soon induces and maintains gentle diaphoresis; the stitch-like pain is relieved; respiration becomes fuller and less hurried; cough grows softer and less hacking; expectoration becomes easy; the temperature steadily declines; the patient soon feels quite comfortable. If pain be very severe, a small hypodermic injection of morphia may be given to adults, although this will, I believe, be rarely required. In the case of children or adults, extract of opium or of belladonna, rubbed up with a little glycerin, may be painted on the skin over the painful part before applying the hot flannel; or tincture of opium, of belladonna, or of aconite root may be sprinkled on the flannel, whenever the attending physician should judge such useful. Children under five years of age will need an emetic of ipecacuanha once or twice a day, if they do not clear the lungs sufficiently by acts of coughing.

* *Pulvis antimonialis* is not James' Powder. It is a worthless substitute for it. See the CHICAGO MEDICAL JOURNAL AND EXAMINER for August, 1879, for a paper by me on True James' Powder

During illness, a light but nutritious diet of good soup, chicken broth, beef tea, and milk with a little bread or crackers, is allowed. If craved, I see no objection to a little chicken, mutton chop, or beef steak broiled. I have permitted such and seen no ill effects result. My chief reliance, however, is placed on milk, which is allowed *ad libitum*. Infants at the breast must depend on the mother's milk. If there be any evidence of prostration or depression, tonics and stimulants are administered. These I rarely had to employ.

The fifteen cases, of which brief abstracts are given below, may be interesting for the following reasons: 1. The disease was typical in all. 2. The beginning of illness was accurately determined in each case. 3. The circumstances and hygienic surroundings were, to all intents and purposes, the same in all. 4. The season of the year (cold weather) was the same in all except three. 5. All the cases were in private practice, and were under my personal care and daily observation from the beginning to the end of illness. 6. Careful physical examination of the chest was practiced daily in each case. 7. The treatment in all was identical and as described above, and was faithfully carried out. 8. In all the disease ended in complete resolution.

The duration of the disease is computed from the outset (which, in the majority, coincided with the time the patient took to the bed) as accurately determined, as I said already, by personal physical exploration of the chest, to the time when *convalescence* was *well established*. In a few cases the nature of the illness was determined before the patient was compelled to keep the bed. Ten, or two-thirds of the cases were children up to twelve years of age, and among whom delirium at night, of an active character, was a prominent feature in about one-half. I append very short notes of each case.

CASE I. Michael M., aged 32, very recently suffered from syphilitic caries of the vomer ending in perforation, walked into my office. Felt chilly. *Lowest lobe of right lung* becoming consolidated. Had rust-colored expectoration. Was six days ill. Recovery complete.

CASE II. Mrs. C., aged 31, multipara, nursing. Was extremely peevish and could not be induced to keep quiet in bed.

To this I attribute the long duration of her illness. *Left lower lobe* involved; had malar blush; was eleven days ill. Recovery complete.

CASE III. Eddie C., aged 4, robust. Had chill. *Lowest lobe in each lung* involved. Was nine days ill. Recovery complete.

CASE IV. Mortimer S., aged 5, recovering from a severe impetigo. Malar blush well marked. *Lowest and middle lobes of right lung* involved. Was six and a half days ill. Recovery complete.

CASE V. Lily H., aged 8, a delicate child. Had chill; well marked malar blush. *Right lowest lobe* involved. Was six days ill. Recovery complete.

CASE VI. Mrs. W., aged 39, multipara (eleven children), very fat, flabby, a beer drinker, recovering from acute rheumatism which induced labor at end of eighth month, five days ago. *Entire right lung* involved. Had rust-colored expectoration; malar blush on right side only. Was seven days ill. Recovery complete.

CASE VII. Emma W., aged 7, delicate, syphilitic (acquired by kissing) for past three years. *Entire right lung and left lower lobe* involved. Had malar blush and herpes at angles of mouth. Was seven and a half days ill. Recovery complete.

CASE VIII. Jennie M., aged 12, very slow, dull child. Had well marked malar blush. *Entire right lung* involved. Was seven and a half days ill. Recovery complete.

CASE IV. Willie C., aged 23 months, robust. Had malar blush. *Right lower lobe* involved. Was six days ill. Recovery complete.

CASE X. James S., aged 6 months, strong, nursing. *Right lowest lobe* involved. Was six days ill. Recovery complete.

CASE XI. William C., aged 30½. Had malar blush and rust-colored expectoration. *Right lowest lobe* involved. Was three days ill. Recovery complete.

CASE XII. Freddie A., aged 14 months, strong. Had chill, malar blush, and rust-colored phlegm got up by emesis. Partially weaned. *Entire right lung* involved. Was six days ill. Recovery complete.

CASE XIII. James S., aged 17 months. Second attack with interval of eleven months health between. (See Case X.) *Left lower lobe* involved this time. Was three and a half days ill. Recovery complete.

CASE XIV. William C., aged 31. Second attack, with interval of six months health between. (See Case XI.) Had chill, malar blush, and rust-colored expectoration. *Entire right lung* involved this time. Was two and a half days ill. Recovery complete.

CASE XV. John G., aged 11, very strong lad. Had malar blush. *Entire right lung* involved. Was three days ill. Recovery complete.

The first of these fifteen cases was seen in November, 1877, and the last in February, 1880—a period of two and a half years. Four cases happened in the month of January, three in February, one in April, two in June, one in August, and four in November; all in the city of Chicago. Although cold may have played some part, possibly the greatest part, in the causation, still I attributed several of the cases to the action of sewer-gas, and have no reason now to alter that opinion. The average initial temperature in each case was 103.2° F.

The shortest mean duration of acute pneumonitis is twelve days according to the writers within my reach. The mean duration of my fifteen cases is six days, with a minimum of two and a half and a maximum of eleven days. From the beginning of illness until the patient was permitted to leave his bed and move about the house, and, in some cases, to go out of doors, was, on an average, seven days.

The relative frequency of the presence or absence of any one particular symptom of the disease can be seen at a glance. The extremely rapid manner in which a lobe or even an entire lung, as seen in cases XI, XIII, XIV, and XV, may become consolidated and just as quickly completely clear up again is interesting and remarkable.

ARTICLE IV.

THE PATHOLOGY AND TREATMENT OF YELLOW FEVER; WITH SOME REMARKS UPON THE NATURE OF ITS CAUSE AND ITS PREVENTION. By H. D. Schmidt, M.D., New Orleans, La. (Continued from page 622, June No., 1881.)

THE STOMACH.

In all serious cases of yellow fever, whether terminating favorably or not, this organ, like the liver, is deeply involved in the pathological process, originally called forth by the introduction of the specific poison of the disease into the blood. In consequence, the clinical symptoms arising from this implication of the stomach, and consisting mainly in a disturbance of its circulation, are, as we have seen, very annoying to the physician, and, at the same time, so distressing to the patient, as to have induced a number of the older physicians and authors to even regard this organ as the true seat of the whole disease. Although it may be justly presumed that the functional disturbances of the stomach are partly depending on the common cause—the presence of the poison in the blood—it is nevertheless true, that the congestion of the gastric veins is only secondary in its nature, depending upon the imperfect portal circulation in the liver. The condition of the stomach, therefore, may in all cases, as I have before hinted at, serve as a true index to that of the liver. And, in order to render the demonstration of the disturbances in the gastric circulation more perspicuous, it may be proper to briefly review the particular blood vessels supplying the mucous membrane of the stomach with blood, and their arrangement.

It will be remembered that the larger blood vessels sent to the stomach form two arches along the so-called curvatures of this organ. The arterial arch along the lesser curvature is formed by the union of the *coronary* artery and of the *pyloric* branch of the hepatic, while that along the greater curvature is formed by the right *gastro-epiploic* branch of the splenic artery; the latter, also, furnishes some small branches, the *vasa brevia*, to the “cul de sac.” The veins, corresponding to these arteries, return

their blood to the splenic and superior mesenteric veins, which by their junction form the portal vein ; sometimes the coronary vein joins the latter directly. The branches arising from these vessels, both arteries and veins, in entering the walls of the stomach, penetrate through its muscular coat to be distributed throughout the sub-mucous tissue in which they divide into subordinate branches. Of these, the finest arterial branches penetrate the muscular layer of the mucous membrane, and arriving at the glandular layer, give rise to a network of capillary vessels with oblong meshes surrounding the individual gastric glands, and extending to the vicinity of their apertures. Here the capillaries of the network somewhat enlarge in diameter, while their meshes assume a more regular polygonal form, each mesh surrounding the aperture of a gland. From these larger capillaries the venous radicles take their origin in the form of minute branches, a small number of which, representing a small district of the mucous membrane, join to form a somewhat larger vessel, which, without receiving any additional branches, descends vertically to join a venous network, extending between the glandular and muscular layers of the mucous membrane. From this network, other branches arise, which, by penetrating the muscular layer, join those larger veins ramifying, in company with the arteries, throughout the sub-mucous tissue, and which finally penetrate the other coats of the stomach to reach their ultimate destination, the splenic, or superior mesenteric vein.

As regards the function of the capillaries of the mucous membrane of the stomach, it may be mentioned that the larger kind, forming polygonal meshes around the apertures of the gastric glands, have been regarded as subserving the process of absorption, while those surrounding the gastric glands themselves, smaller in diameter, and forming oblong meshes, are considered to furnish the material for the secretion (*Frey*). We may now proceed with the description of the pathological changes taking place in the mucous membrane of this organ.

In almost all cases of yellow fever, this membrane is found more or less congested. The congestion, however, does not extend uniformly throughout the whole membrane, or even larger portions of it, but is confined to smaller or larger spots or dis-

tricts, in which it is observed to proceed from one or more centers. From these centers it extends or radiates in a lesser degree, either gradually to be lost, or to pass over to another congested district. It is owing to this peculiarity of the congestion that it presents no uniformity of character, but is observed to spread irregularly over larger or smaller portions of the membrane. Neither are the congested portions of the latter limited to any particular region of the organ; they may be nearer to the cardiac, or to the pyloric extremity; in most instances, however, they are found in that portion forming the greater curvature. In examining the congested portions of the mucous membrane with a loupe, magnifying about five diameters, the congestion will be found confined to the minute venules, presenting an arborescent arrangement resembling the broken meshes of a vascular network (Fig. 6, *a*). From the center of each individual district, corresponding to the apparent trunk of these minute vessels, a few fine branches are seen to proceed, which in their turn give rise to still finer ones, the whole resembling the branch of a tree without leaves. From our description of the arrangement of the blood vessels of the mucous membrane of the stomach, given above, it may be inferred that the finest of these vessels are identical with those minute venules arising in the upper and larger capillaries, surrounding the apertures of the gastric glands, and that the branches, formed by their junction, converge to a common center, in order to form one of those larger vessels, which, without receiving any additional branches, descend between the gastric glands (Fig. 5) to join their respective venous network extending between the glandular and muscular layer of the mucous membrane.

This particular form of congestion prevails in a greater or lesser degree in almost all the congested portions of the membrane. Each descending venule or vein, as has been seen, forms one of the centers, while its minute tributaries represent the districts. Nevertheless, in most fatal cases, besides this particular form of congestion, a number of small and defined spots or patches are also found, presenting a distinct red color, and resembling small ecchymoses or extravasations of blood. Their size is very limited, ranging from that of a mere dot to about

two or three mm., seldom more; and if they are examined with a strong loupe, they are found to consist of an unbroken network of minute vessels, congested with blood (Fig. 6, *b*), and identical with that network of large capillaries which surrounds the apertures of the gastric glands. In most instances, these small congested spots or patches are found between or along the sides of the plicæ, sometimes extending over the summit.

In examining thin sections made vertically through one of these small congested patches, the peculiar character of the congestion can be clearly demonstrated. In such sections (Fig. 5), then, it will be found that, while the arteries with their *capillaries*, surrounding the gastric glands throughout nearly their entire length, are *empty*, all the veins throughout the entire wall of the congested portion of the stomach are congested with blood. The congestion commences in the network of large capillaries, surrounding the apertures of the glands, whence it extends through the venous radicles to all other veins of the organ, contained in the section. These large capillaries, especially, are distended with blood corpuscles, which, in a number of places, may be observed to have ruptured the minute vessels, and extravasated into the surrounding tissue. In other places, again, no rupture has taken place, but, in consequence of the stasis of blood, which obviously must have existed in these localities, the blood corpuscles have parted with their hæmoglobin, which, passing through the walls of the vessels, was absorbed by the neighboring epithelial cells, as shown by the brown color they present to the eye of the observer. Thin sections, sliced off with a sharp knife from the surface of one of these spots, embracing the outer epithelium, also show distinctly the condition of the membrane just described. It will be easily understood that the hæmorrhagic effusions into the stomach, occurring during the course of yellow fever, and represented by the "black vomit," are derived from the rupture of these minute vessels. In the numerous sections of the mucous membrane of the stomach which I have examined, I have failed to discover any product of inflammation.

It has been asserted, and is believed by a number of physicians, that in yellow fever the epithelium of the stomach underwent fatty degeneration. During the epidemic of 1867, but

particularly during that of 1878, I had carefully examined the fresh epithelium of a considerable number of stomachs without finding any traces of this degenerative process. Nor can it be demonstrated in the thin sections of the hardened specimen, in which the details of the cellular elements can be minutely studied. On the contrary, it is not only found that the epithelial and glandular cells contain no fat globules, but, moreover, that their protoplasm is very readily colored by the staining liquid.

With the exception of the pathological changes above described, none are observed in the remaining tissues of the stomach.

THE INTESTINES.

The distribution and arrangement of the blood-vessels of these organs being very nearly the same as that met with in the stomach, it may be expected that the congestion of these parts also partakes of the same character. But though, in the greatest number of fatal cases, the larger veins, emerging from the walls of the intestines to become tributaries of the portal vein, are found filled with blood, the congestion of the minute vessels of the mucous membrane very rarely attains as high a degree as in the same membrane of the stomach. An explanation of this fact may be found in the distance intervening between the portal vein and the mucous membrane of the intestines, being greater than that existing between this vessel and the mucous membrane of the stomach. The first effect of any retardation of the portal circulation of the liver will, of course, be felt in those tributaries which the portal vein receives nearest to this organ. For this reason, hæmorrhages into the intestines only take place in very severe cases, though a congestion of the venous radicles is frequently observed, especially in the upper portion of the small intestines. When blood is observed to pass from the bowels, however, it does not always signify that a hæmorrhage from the mucous membrane of the intestines has occurred, as it may possibly be derived from a hæmorrhage into the stomach, and, instead of being vomited, be discharged by way of these organs. The appearance which the congested mucous membrane of the intestines presents to the eye of the observer is very similar to that of the congested portions of the stomach above described, having its seat in the minute venous radicles. Those small red spots or patches, formed by a

congestion of the larger capillaries, surrounding the apertures of the glands, are rarely met with upon the mucous membrane of the intestines.

THE SPLEEN.

This organ is almost always found in a normal condition; any deviation from the latter should, as in the case of the lungs, be considered as having no relation with yellow fever, but depending upon other causes. The only pathological phenomenon which the microscopical examination in some cases reveals, consists in the presence of small masses of black pigment, deposited in the pulp of this organ, but which are frequently met with in malarial spleens and livers. The blood-corpuscles, neither, present anything remarkable in their form, or otherwise. These statements are based upon the examination of the fresh pulp in a number of cases, and, furthermore, upon that of thin microscopical sections of the hardened specimen.

THE KIDNEYS.

In almost all fatal cases of yellow fever, the microscopical examination shows that certain pathological changes have taken place in the parenchyma of these organs. These changes, as I shall show directly, chiefly consist in a degeneration of the epithelium lining the uriniferous tubules, and, in some instances, they exist to so small an extent as not to alter the normal appearances presented by the cut surfaces of the kidneys to the naked eye. Among the autopsies which I performed during the epidemic of 1878, I met with a number of such specimens of kidney, the normal appearance of which so much misled me as to make me indifferent to their preservation. Subsequent microscopical examinations of thin sections made from similar specimens, however, showed me that even in these kidneys of normal appearance the peculiar changes do exist, and are easily demonstrated; and, in consideration of their almost constant occurrence, they may safely be regarded as characteristic of yellow fever.

As the anatomical construction of the kidneys is quite complicated, I do not consider it out of place to briefly review it before proceeding to the description and discussion of the pathological phenomena observed in these organs.

When a kidney is divided longitudinally, the cut surface appears as if consisting of two different substances. The one forming the external portion of the organ, and accordingly called "cortical," presents a granular appearance, while the other, called "medullary," is collected in conical or pyramidal masses, and appears to consist of bundles of fibers, converging in each pyramid from the base to the apex. Thus the kidney may be regarded as composed of the cortical substance and about a dozen, or more, of these conical bodies, the bases of which are connected and surrounded by the former, while their apices, represented by the *renal papillæ*, project into the cavity, or *sinus*, of the organ; and, furthermore, each pyramid, together with its respective portion of cortical substance, may be looked upon as a separate gland. In embryonic life, the kidney really consists of a number of minor kidneys, held together by connective tissue, which subsequently become blended with each other. The secreting portion of the kidney is represented by an immense number of tubules, lined by a layer of epithelial cells, the true secreting elements. The mutual relationship of these tubules is quite simple, consisting in the junction which a number of the primary tubules form with a particular kind of straight or collecting tubules, and in the union of these to form, finally, still larger ones, the *ductus papillares*; but the changes which they undergo in their diameter and in the direction of their course, together with the peculiar arrangement and distribution of the blood-vessels, render the study of the renal parenchyma rather complicated. The apparent difference existing between the cortical and medullary substances is owing to these variations existing in the diameter, form, and course of the uriniferous tubules, and in the arrangement of their minute blood-vessels, though a distinction may be presumed to exist in their functions; that is, the cortical substance representing the proper secreting portion of the gland, while the medullary, consisting entirely of straight and larger tubules, represents that portion through which the secretion is discharged.

If the cut surface of the cortical substance is examined with a sufficiently strong loupe, a considerable number of minute spherical bodies will be observed; these are the so-called "Malpighian

bodies," upon which the uriniferous tubules commence. Each of these bodies consists of a conglomeration of minute vessels, surrounded by a thin capsule, the continuation of which, in the form of a short and narrow constriction or neck, represents the commencement of a uriniferous tubule. The capsule itself, consists only of a layer of flat pavement cells (endothelium), surrounded by a thin layer of connective tissue, while the tubule, throughout its entire length, represents a tube of a structureless so-called basement-membrane, lined by an epithelial layer of cells, which, in different portions of the tubule, assume a different form and arrangement. The tubule, after having arisen by its neck from the capsule, rapidly enlarges in diameter to form a comparatively wide tube, which, bending into several convolutions, pursues its course in a slightly oblique direction toward the center of the kidney; but, diminishing again in diameter to about one-third of that of the convoluted portion, it suddenly makes a turn and descends vertically toward the medullary substance, forming thus what is called the *descending limb* of the tubule. After penetrating, to a lesser or greater distance, into the medullary substance, it makes, opposite to the side whence it came, a short bend upon itself (loop of *Henle*), and ascends again in a vertical direction, forming the *ascending limb*. Having passed beyond the height of its respective Malpighian body, it again enlarges in diameter, and forms the *intermediate* portion or canal, consisting of a few convolutions. This portion, after being once more reduced in diameter to a very fine tubule, makes a turn sideward to join its respective *collecting* tubule. The collecting tubules commence near the surface of the kidney, where they receive a limited number of primary tubules, and thence pass, without receiving any others, in a direct vertical direction into the medullary substance. Here, in uniting with others of their own kind, they form larger tubules from which, by successive union, the ductus papillares finally result.

The initial or convoluted portion of the uriniferous tubule is lined by an epithelium consisting of cells of a turbid appearance, and of a slightly conical form, resting with their bases upon the interior surface of the basement-membrane of the tube, and presenting their smallest surfaces (the apices) to the lumen of the tube. In

the narrow descending limb, to some distance beyond the loop of Henle, the epithelium consists of smaller cells arranged in the form of a pavement, their nuclei projecting into the interior of the tube. In the ascending limb, the cells assume a somewhat squamous arrangement up to the intermediate canal, which is lined by the same epithelium as found in the convoluted portion of the tubule. The epithelium found in the collecting tubules, and in those larger ones formed by the latter, consists of slightly conical cells, presenting their smallest surfaces to the lumen of the tube.

In the same manner as the kidney is composed of a certain number of conical or pyramidal bodies, these, on their part, consist of a considerable number of primitive cones formed by bundles of collecting tubules, together with the primary tubules they receive. These bundles may be regarded as separate lobules of the gland, for all the collecting tubules of one bundle are received by one and the same of the larger tubules, by which, finally, the ductus papillares are formed. And it is by these separate bundles of collecting tubules that the peculiar striated appearance, observed on the cut surface of the organ, is produced, the striæ being formed by the bundles and their interspaces. In the latter, the blood vessels are lodged.

The distribution of the blood vessels in the kidney is rather peculiar. The smallest branches, resulting from a successive division of the renal artery, proceed at once to the inner border of the cortical substance. Here they give rise to two sets of minute vessels—the *inter-lobular* (arteriæ interlobulares) and the *straight* arteries (arteriæ rectæ), which proceed in opposite directions. The interlobular arteries enter the cortical substance, and, lodged in the interspaces of the primitive cones, pursue their course toward the surface of the kidney, while, at the same time, they send minute arterial twigs (vasa afferentia) to the Malpighian bodies. Each of these arterioles penetrates the capsule of one of the latter, and then divides into a limited number of capillary vessels, which, after having formed a number of loops among themselves, reunite to form again another minute vessel (vasa efferentia), which leaves the capsule near the place at which the afferent vessel had entered. The conglomeration of vessels thus

formed in the interior of the capsule represents the so-called "glomerulus." After having left the glomerulus, the efferent vessel terminates in the true capillary network surrounding the uriniferous tubules of the cortical substance, and giving rise to the radicles of interlobular veins.

The straight arteries, arising by a short trunk from the same renal branch as the interlobular, enter the medullary substance, in which, lodged in the interspaces of the primitive bundles of tubules, they give rise, successively, to small bundles of very fine straight vessels, the subordinate branches of which terminate in the regular capillary network, surrounding the tubules of the medullary substance, and consisting of long meshes. The vasa efferentia of the glomeruli near the border of the cortical substance, also, join this network of capillaries, in which the venous radicles of the straight veins arise.

Before dismissing the anatomical construction of the renal parenchyma, it may be further remarked that the glomerulus has no direct connection with the interior surface of the capsule, being only suspended in the cavity of the latter by its root, formed by the afferent and efferent vessel; neither does the liquid contained in the cavity directly touch the walls of the capillary vessels, they being covered by a layer of delicate cells, the nuclei of which are conspicuously distributed over the entire glomerulus.

While in the parenchyma of the liver the degenerative process, as we have seen, chiefly consists in an infiltration of fatty matters, derived from the blood and absorbed by the hepatic cells, we find it represented in the kidney by a true degeneration of the protoplasm of the epithelial cells lining the uriniferous tubules. The pathological process in the kidney appears to be initiated, as in the liver, by a general hyperæmia of the organ, the traces of which may still be detected by the microscopical examination. But, as in most fatal cases of yellow fever the congestion has, at the time of death, become greatly diminished by the ensuing degeneration, it is not often, when the autopsy is made, that the kidneys present a highly congested condition. Nevertheless, I have met, in former epidemics, with a limited number of cases, in which the kidneys presented the reddish-blue color of congestion to a considerable degree. And even in those kidneys

in which the degenerative changes have already become perceptible to a microscopical examination, some portions of the organ may still be found in a state of hyperæmia. The traces of the congestive stage of the kidneys are best studied in thin sections. The microscopical examination of these sections shows that, in most cases, the congestion was at the time of death confined to the straight and interlobular veins, which are still found filled with blood corpuscles. However, not unfrequently a number of the capillary vessels, especially those along the border of the medullary substance (*Grenzschicht* of *Ludwig*), forming oblong meshes, are also found filled with blood. And, moreover, in some instances, I have even observed the minute vessels of a number of glomeruli in the same condition, indicating that the congestion must have extended throughout the capillary network of the cortical substance; and, in considering the contraction of the minute arteries, very probably taken place at the time of death, it may be presumed that, even in those cases in which only the minute veins were found filled, the congestion had likewise existed in the capillary network before the fatal issue took place. The presence of hæmoglobin, which in a number of cases may be demonstrated in the epithelium of the uriniferous tubules, moreover, corroborates the latter supposition.

The degenerative process, taking place in the renal parenchyma during the course of yellow fever, is in some respects peculiar, and appears not to resemble, in all points, the fatty degeneration of the kidney observed in connection with parenchymatous nephritis. The chief part of the process rather consists in a gradual breaking down and dissolution of the epithelial cells of an indefinite number of uriniferous tubules, terminating in most instances in a fatty metamorphosis of the remains. As a result of the degeneration, numerous so-called albuminous cylinders, and other infarctions, differing in composition and form, are formed in the interior of a considerable number of uriniferous tubules. These urinary infarctions, met with in certain organic affections of the kidney, have, since their discovery, always attracted the attention of pathologists, and as the probable origin and mode of formation seems to be still an open question, I shall endeavor to render a true account of their appearance in the yellow fever

kidney, and also of their relationship to the degenerated epithelial cells. In doing so, I shall discuss the most prominent points of the question with special reference to their bearing upon the pathology of yellow fever. But, in order to facilitate the description of the condition and probable nature of the degenerated cells, and of the infarctions, I should make some preceding remarks upon the advantages derived from staining the thin sections of kidney with carmine, a valuable accessory in these studies, for the purpose of discriminating correctly between the different degrees of degeneration in which the cells may be affected, and also for determining the particular nature of the different forms of infarctions.

Knowing, namely, that the absorptive power of an organic cell is proportionate to the normal condition of its protoplasm, the extent or degree of the degenerative process, going on in the latter, may be determined by the degree of the coloring. The epithelial cells of the uriniferous tubules, therefore, will be perfectly colored as long as they are in their normal condition, but as soon as their protoplasm commences to degenerate, their absorptive power diminishes, and they appear only faintly colored; or, if the process of degeneration has farther advanced, the protoplasm remains colorless, while the nucleus may still be colored, or, if likewise degenerated, remain uncolored. The fatty matters resulting from the metamorphosis, remain, of course, uncolored, and appear yellowish when seen under the microscope. The so-called albuminous cylinders possess a considerable power of absorption, and appear highly colored. If hæmoglobin is present in the cells, they assume, in proportion to its quantity a more or less brown color; in relation to this body, especially, the carmine is preferable to other colors.

The infarctions formed in the uriniferous tubules during the course of yellow fever, differ somewhat from each other in their origin and formation. A number of them represent albuminous cylinders, while others consist of the remains of degenerated epithelial cells, or may even be composed of both. They are met with in all portions of the uriniferous tubules, and in correspondence with the diameter of these canals, vary in thickness. The largest are found in the convoluted portions, the so-called tubuli

contorti, and in the intermediate canals (Schaltstuecke of *Schweigger-Seidel*), which they frequently fill up throughout their entire length. In the ascending and descending limbs and in the collecting tubules, they only exceptionally attain a great length, being generally short in extent. The relative number in which they are found differs considerably in different kidneys, and is proportionate to the extent of the pathological condition of the organ, which, as has been remarked before, greatly differs in different cases. Neither do the particular forms of infarctions stand in any fixed relation to the extent of the destructive changes in the kidney, for, while a considerable number of the simple albuminous cylinders may be met with in one case in which these changes exist to a great extent, they may hardly be observed in another case, in which only a comparatively small number of uriniferous tubules may be affected by the degeneration, or be obstructed by another form of infarction. In the same way does the degree of the degenerative process not always correspond to the extent in the organ, for there are cases met with in which few, or even no albuminous cylinders are observed, and in which the epithelium of only a small number of uriniferous tubules are found to undergo degeneration, but in which the latter process has nevertheless arrived at the fatty metamorphosis,—while there are other cases again, in which the formation of fat is hardly observed. To explain this discrepancy in the extent or degree of the pathological changes in the yellow fever kidneys, the comparative length of time preceding the fatal issue, and also the severity of the attack, must be considered; for, in those cases of a rapid course, and in which the patient succumbs in a few days from the effects of the cerebral disturbances, the kidneys may be found affected to only a small extent, while in other, more protracted cases, more time is afforded to the pathological process to extend farther throughout the organ.

Before proceeding to the description of the infarctions, a previous examination of the particular condition of the epithelium of the uriniferous tubules, which not only furnishes the material to the greater number of the obstructions, but, moreover is probably otherwise instrumental in their formation, may not be considered out of place. And, for this purpose, I shall refer in my

description to one of those cases, in which the pathological changes observed are decidedly pronounced.

In examining a very thin section of kidney of such a case (Fig. 7), perfectly stained with carmine, it is found that the epithelium in the tubules has considerably diminished in its normal thickness (*b*). This observation is made as well on the entire tubule as on the transverse section of it. In the convoluted portions of the tubules, especially those surfaces of the cells directed toward the lumen, and which in the normal condition are conical, are now observed to be flat, causing the cells to appear rectangular in form. The lumen of such a tubule, as it is seen in a transverse section, therefore, will be found much larger than it is in the normal condition, though the epithelium may be intact; this is owing to the cells having lost their turgescence and conical appearance. In many cells, especially when viewed in profile, the nucleus appears more deeply colored than the protoplasm of the cell, while in others the color is more evenly diffused throughout the whole. In the greater number of tubules, the epithelium appears intact, but there are many others in which it is detached from the basement-membrane, leaving an interspace between it and the latter. In a number of other tubules, portions of the epithelium are observed to be completely separated from the basement-membrane, and to project into the lumen (Figs. 7, 8 and 9), or, its cells may be seen separating from each other, singly, or in small shreds. In some tubules, finally, the entire epithelium appears broken up, and with the exception of a small number of very faintly colored, or colorless cells, still adhering to the basement-membrane, either in small groups or singly, the whole tubule appears empty.

The cells of the epithelium, however, while undergoing this process of atrophy and disintegration, show as yet no traces of fatty metamorphosis. Most of those, still in contact with the basement-membrane, and, in severe cases, even forming the greater portion in the section, have absorbed carmine, especially their nuclei. But as soon as they are detached from this membrane, their power of absorption diminishes, and they appear but very faintly colored; or even, while the nucleus may still present a feeble carmine tint, the protoplasm of the cell has remained

uncolored. It is not until they separate from each other that their degeneration and disintegration is really perceptible. The destructive process appears to consist in a dissolution or melting down of the hyaline portion of the protoplasm, liberating the granules, which, together with the hyaline substance, finally undergo a fatty metamorphosis (Figs. 11 to 15). At any rate, the granular remains of these cells do, at first, not always present a fatty appearance, though, soon after, the dark contours and the refractive property of the minute granules give sufficient evidence of their fatty nature. Very often, larger or smaller fragments of cells are observed to form a part of these remains, indicating again the grouped arrangement of the granules of the protoplasm, to which I referred in connection with the hepatic cells. In consequence of the breaking down of the protoplasm of the cells, the nuclei are set free, and traveling along with the general mass, they form a part of the resulting infarctions, in which they are almost always observed, either singly, or in clusters. They are then distinguished by being higher colored than the rest of the accumulation.

It is a remarkable fact that, during this process of fatty metamorphosis, taking place in the epithelium of the uriniferous tubules, it is quite rare that larger fat-globules are met with. I have examined a great number of sections for this special object, and have also found larger fat-globules in some of the tubules, especially in those near the capsule; but the number of these instances is so limited that their occurrence may rather be regarded as an exception to the rule. The comparative rarity, or absence, of larger fat-globules may be explained by the constant flow of some urine, even through these obstructed tubules, interfering with their formation. This supposition is corroborated by the streaked, thread-like appearance of the fat as it is observed in the interior of the tubules, particularly directly below the infarctions. Here it appears in long streaks of a yellowish color (Fig. 14), forming quite a contrast to the carmine coloring of the cells. Minute fatty granules, distinguished by their dark contours and high refraction, together with other cellular debris, are observed with these streaks.

In consequence of the degeneration and disintegration of the epithelium, many portions of the tubules are met with entirely denuded of their epithelial lining (Figs. 7 and 8, *d*). Although in the convoluted portions this condition is also observed, it is, nevertheless, more frequently found in the smaller tubules; and it is remarkable that not only single tubules are met with stripped in this manner of their epithelium, but generally a whole group or bundle, including descending, ascending, and collecting tubules. The only explanation which I can find for this phenomenon is that these groups represent certain arterial districts. But, even in these empty tubules, epithelial cells, singly or in patches, together with accumulations of granules and nuclei, may frequently be observed. In most instances, perhaps, the empty tubules have preserved their normal caliber, though not unfrequently groups are met with in a collapsed condition, their basement-membrane presenting faint outlines.

Besides the condition of atrophy and degeneration just described, in which the epithelial cells of the uriniferous tubules are found in yellow fever, there is another condition, indicated by a peculiar appearance of a considerable number of cells, and which seems to precede that of atrophy and degeneration. The special description and consideration of this condition, however, I must postpone until I have described the appearance and the nature of the infarctions.

These infarctions, besides differing from each other in composition, are met with in the uriniferous tubules in different conditions. The so-called albuminous cylinders, when examined in an uncolored section, appear under the microscope finely granular, amorphous, faintly glistening, and of a yellowish color. They are, however, studied to a greater advantage in sections stained with carmine, in which, with the exception of the color, they present, especially in transverse sections of the tubules, the same characters. Possessing, as already mentioned, the power of absorbing coloring materials in a very high degree, they appear in such sections very highly colored, and exhibit the carmine tint with a greater brilliancy and intensity than the normal protoplasm of the cells; therefore, when they are met with in their pure condition, that is, unmixed with epithelial fragments and

remains (Fig. 7, *a*), they are easily recognized by their brilliant and even coloring. They generally fill up the whole lumen of the tubule, and mostly terminate in tapering, rounded-off extremities. In most instances, perhaps, they are still surrounded by the epithelium, though not unfrequently they are met with in denuded portions of the tubules, and in direct contact with the basement-membrane. When covered by epithelium, the outlines of the cells may still be recognized, though the whole cylinder appears somewhat darker. Many of these cylinders are found in a broken condition, either simply fractured across, or into a number of greater or smaller fragments of rectangular or polyhedral forms. It is difficult to determine the cause of these fractures, though it may be safely presumed that they occurred by the instrumentality of the knife during the cutting of the sections. In the study of these sections, therefore, the investigator should take this circumstance into account, in order to guard against false conclusions, which he otherwise might draw from his observation. There are, nevertheless, specimens of these broken cylinders here and there met with, of which the fragments appear to have been removed from each other after the occurrence of the fracture, while their angular borders appear slightly rounded, as if washed away by the passage of the urine. Whatever the cause of these fractures may be, they, themselves, at least, give evidence of the brittle nature of these cylinders, which, accordingly, bear resemblance to the so-called "waxy cylinders," frequently met with in interstitial nephritis. A number of these cylinders are observed, which appear distinctly granular in their composition, the very minute granules exhibiting dark contours. Not unfrequently the lower end of the cylinder is in a state of dissolution, when the individual granules may be distinctly seen. And if the very border of the disintegrating extremity is closely examined, some of the granules may almost always be observed exhibiting their fatty nature by their refractive property and dark contours.

Perhaps the greater portion of these cylinders is found to contain epithelial remains, consisting of granules, nuclei, and even entire cells, or their fragments (Fig. 8, *a*). Mostly, their morphological elements of the epithelium are irregularly mixed up into a shapeless mass, but very frequently cylinders are observed

containing, besides the granules, a whole cluster of nuclei, or even entire cells. If these fragmentary remains of the epithelium have, as yet, not undergone the fatty metamorphosis, they appear more intensely colored than the original substance of the cylinder itself, and exhibit very dark contours and a peculiar luster. But not unfrequently instances are met with in which a portion, or the whole, of these epithelial remains have already undergone this metamorphosis, and accordingly appear lighter, or even yellowish, but also with dark contours, especially the granules (Fig. 15, *a*). The degenerated epithelial elements, not enclosed in the cylinder, of course, exhibit distinctly the fatty appearance, as before described. As regards the relative proportion of the albuminous substance of the cylinders with the epithelial remains, it, of course, must differ in every combination of cylinder formed, and be subject to much variation. It may be presumed that the epithelial elements have accumulated in the lumen of the tubule previous to their saturation by the albuminous substance, to which the intense coloring must be attributed. Judging from the tapering form of the extremities of these mixed cylinders, and also from the irregular outlines observed on some of them, they appear to be prone to a gradual liquefaction.

A third kind of infarctions met with in the uriniferous tubules, in yellow fever, are those solely composed of the disintegrated and degenerated epithelial elements. They consist of granular masses of irregular forms, derived from the degenerated cells, and mostly enclosing free nuclei and cells, or their fragments; sometimes, however, clusters of entire nuclei and cells are observed (Fig. 8, *e*). The size or extent of these accumulations is very variable, but it may be stated that, as long as they remain free from being saturated by the albuminous liquid, they never assume the same dimensions as the albuminous cylinders above described, though they are met with in the same localities as the latter. Neither do they appear to become very easily stationary in one particular place of the tubule; on the contrary, being of a softer consistence, and more yielding, and, furthermore, undergoing easily fatty metamorphosis, they are carried along by the pressure of the urine. These epithelial accumulations are frequently observed in a state of fatty degeneration, the whole mass

appearing yellow, or, sometimes, yellowish mixed with brown; in the latter instances the brown color represents hæmoglobin, previously absorbed by the protoplasm of the cells.

Having thus far described the degenerated condition of the epithelium, lining the uriniferous tubules, and the peculiarities of the different kinds of infarctions, formed in the latter during the comparatively brief course of yellow fever, I shall now consider the probable origin and mode of formation of these infarctions, and, moreover, briefly review the existing theories on the subject.

The old theory of the mode of formation of the so-called albuminous cylinders referred the process to a transudation of the albumen of the blood through the walls and the covering cells of the minute vessels of the glomeruli into the uriniferous tubules, in the interior of which the cylinders were formed by a precipitation of the albumen from the urine. This theory is entertained by perhaps the majority of pathologists, though the other, more recent, according to which the cylinders represent a product of secretion from the epithelial cells, or are even the result of a transformation of these cells themselves, is also supported by a number of distinguished authorities. To myself, the old theory hitherto appeared sufficiently plausible to explain the whole phenomenon, though I must confess that an observation which I made during these studies on a considerable number of epithelial cells, to be described hereafter, has induced me to view the more recent theory in a different light. Let us, therefore, examine both sides of the question.

As the old theory is sufficiently understood to pass without further explanation, I shall, in support of it, only cite the views of *Runeberg*,* one of the more recent investigators into the pathogenetic condition of albuminuria. They are as follows: "The transudation of serum albumen always takes place in the glomeruli. It is conditioned by an increased permeability of the walls of the looped vessels of the glomeruli and of the epithelium covering them. In consequence, the particles of albumen suspended in the serum of the blood, which under normal conditions

* *Virchow u. Hirsch, Jahresbericht etc., für das Jahr 1878, Vol. I, p. 219.*

are unable to pass through the membranes of the glomeruli, may now in part be filtered with the remaining constituents of the urine. This increase of permeability is, in otherwise healthy kidneys, already caused by a considerable diminution of the difference between the pressure of the blood inside the glomeruli and the counter pressure existing in the uriniferous tubules. The accidental or transitory albuminuria is, therefore, conditioned by a considerable augmentation of the presence of the blood in the glomeruli, or by a diminution of the pressure in the tubules, or by both of these circumstances. In the permanent albuminuria, however, increased permeability of the filtering membrane is conditioned by an inflammatory or degenerative process affecting the vessels of the glomeruli; but, even here, the permeability is perceptibly influenced by the relative amount of pressure, and, in the same direction, also the amount of albumen in the urine, as explained before. A part of the albuminous substances, as the egg albumen and the hæmoglobin, are in a higher degree filterable than the serum albumen. As soon, therefore, as these substances are, in any way, mixed with the serum of the blood, they at once, even under normal conditions of the pressure of the blood, filter into the urine like the soluble salts."

The more recent theory, according to which the cylinders represent a product of secretion from the epithelial cells of the uriniferous tubules, or result from a transformation of these cells themselves, has been advocated by *Oedmansson*, *Axel Key*, *Oertel*, *Rovida*, *Senator*, *Heynsius*, *Bayer*, *Birch-Hirschfeld*, *Aufrecht*, and probably by a number of other pathologists. I shall cite the views of some of these authors. Thus, *Rovida** who subjected the urinary cylinders to an extensive chemical examination distinguishes three particular kinds, the *colorless*, the *yellow*, and the *epithelial*. As regards the chemical nature of the colorless, and the yellow or waxy cylinders, he says, that they do not represent albumin, or an albuminate, nor one of the known bodies derived from albumen, although they chemically resemble the latter substances.

* l. c.—für das Jahr 1872, Vol. I, p. 206.

He states a case of diffused nephritis without contraction or amyloid degeneration of the kidney, in which, beside colorless and yellow cylinders, numerous yellowish minute scales, exhibiting a similar luster as the latter, had been observed in the urine during the life of the patient. The same elements were met with in the fresh kidney, when microscopically examined after death. After the organ had been hardened in Mueller's fluid and in alcohol, the epithelium of the convoluted tubules appeared turbid and granular, so that the nuclei could scarcely be distinguished; the lumen was filled up, partly with homogeneous, partly with slightly granular globules of the same color and refractive as the yellow cylinders. In many uriniferous tubules, spherical drops of yellow color were observed, especially in transverse sections, to project from the epithelium; they were also found, here and there, in the lumen, partially collected into irregular polyhedral figures. In other places they formed an almost spherical contour, blending toward the center with a firmer, more compact mass, which, filling up more or less perfectly the lumen of the uriniferous tubule, represented a yellow urinary cylinder. From this observation, Rovida concludes, that the yellow cylinders, also, are products of secretion from the epithelium of the uriniferous tubules, the same as had been previously shown by Oedmansson, Key, and Oertel in connection with the colorless cylinders. But, finding in the same kidney, in some places, especially in the descending and ascending tubules and in the loop of Henle, the epithelium yellowish and more refracting, he agrees with Key, in admitting that the cylinders may also be formed by a fusion of transformed cells.

Senator,* who also made numerous investigations upon the albuminous bodies in the urine, regards albuminuria as depending upon an abnormal circulation. The increase of tension which the vessels of the glomeruli experience during a general venous congestion, he thinks, is inferior in degree to that of all other capillaries, while at the same time the secretion is diminished by an accumulation of the secreted matters in the uriniferous tubules. In addition, a diminution of the arterial pressure,

* l. c.—für das Jahr 1874, Vol. I, p. 342.

as usually observed during venous congestion, occurs. Therefore, the albumen cannot be regarded as a product of filtration from the glomeruli, but is probably derived from the interstitial vessels bearing high pressure. In pure cases of amyloid degeneration, the urine voided from the Malpighian canals must be regarded as a mixture of a serous, non-inflammatory transudation, pressed through the glomeruli. The changes, occurring in the different forms of diffused, interstitial and parenchymatous nephritis in the urine, are depending upon the effects of the changed afflux and reflux of the blood in the vessels of the glomeruli, and in the interstitial vessels, and from those caused by the accumulation of secreted matters in the uriniferous tubules. The albuminous cylinders Senator regards as a product of a disturbance of nutrition of the epithelium.

Heynsius,* who closely investigated the same subject, agrees with Senator in the following points: namely, that the albuminous bodies met in the urine need not resemble those of the plasma of the blood in all points; that the epithelium of the kidney undoubtedly possesses some influence over the secretion in general, and over that of the albumen in particular; and that the cylinders are not formed by transudation from the plasma of the blood, but are rather derived from the protoplasm of the epithelium.

The cause of albuminuria, he thinks, may be found not only in an increased venous pressure, but also in a diseased condition of the epithelium of the uriniferous tubules, in consequence of which the quantity of albumen, transuding through the vessels in the normal condition, is not consumed for the nutrition of the epithelium. The destruction of the latter, however, causes a diminution of the acid reaction, followed by an increase in the quantity of albumen.

Birch-Hirschfeld† adopts the view of Key, according to which the hyaline cylinders are probably formed by a secretion of the epithelium, while the granular cylinders are formed by a fusion of the degenerated epithelial cells.

Aufrecht‡ found that in the rabbit, after ligation of the ureter on one side, a granular turbidity of the epithelial cells in the

* l. c. p. 243.

† *Birch-Hirschfeld*.—"Lehrbuch der Pathologischen Anatomie," 1877, p. 1038.

‡ *Virchow u. Hirsch*, Jahresbericht, etc., f. d. Jahr 1878, Vol. I, p. 222.

cortical substance of the kidney of the same side, associated with the formation of cylinders takes place, followed—about six days after the ligation—by a proliferation of cells in the interstices of the uriniferous tubules. He agrees with Oedmansson, Axel Key, Senator, and others, that the cylinders are formed by the irritated epithelial cells, and represent a secretion, which, in several instances, he has seen projecting from the cells.

Having thus briefly reviewed the leading theories on the formation of the albuminous cylinders, and, moreover, cited the particular views of some of the investigators, I shall now proceed to state my own observations on the subject. In the preceding description of the condition of the epithelium of the uriniferous tubules, the degree of degeneration in which the cells were found, was, as will be remembered, determined by the capacity of absorption which they showed for coloring matters. In accordance with this test, those least, or not at all, degenerated, exhibited a perfect staining, while others were more imperfectly colored, or had remained entirely colorless. These cells, moreover, without reference to the quantity of carmine absorbed, did not present any lustrous or glistening appearance, nor that intense color exhibited by the urinary infarctions. There are, however, a considerable number of others, as yet not mentioned, met with in all sections of yellow fever kidneys, which do present *the same intense color and glistening aspect as the infarctions*. They are particularly well exhibited in transverse sections of the uriniferous tubules in which the epithelium appears in the form of a ring, though they are also distinguished in others, or even in the entire tubule through the basement-membrane. In many instances, only a certain number of the cells, forming the ring, present this peculiar appearance (Fig. 9), though in others, all the cells of the epithelial ring appear affected (Fig. 10).

These deeply colored and glistening cells are not confined to any particular locality, but are met with in all portions of the tubules. Neither are they confined to particular cases, for I observed them in all the sections I examined, and without reference to the greater or lesser extent of the destructive changes in the parenchyma of the kidney. They were rather more frequently observed in the milder cases, in which the process of

degeneration was of a limited extent. In such cases, I also observed this characteristic appearance on a number of cells, lining some of the collecting tubules, and which showed no signs of atrophy or degeneration besides (Fig. 10, *a*), but had preserved their normal size and form. In some of these sections, no cylinders of notable size were found; those met with were contained in the smaller tubules and very short. In these instances, however, a number of transverse sections of the smaller tubules were also observed, in which not only the epithelial ring exhibited the deeply stained and glistening appearance, but in which the lumen was moreover filled with a cylinder (Fig. 10 *b*), presenting the appearance of being blended with the ring, such as was observed by Rovida in the uncolored specimen.

It will be obvious that these observations strongly corroborate the more recent theory, according to which the albuminous cylinders are a product of secretion from the epithelial cells of the uriniferous tubules, or may even be formed by a fusion of the degenerated cells themselves. Some additional remarks, however, will be required to render the probable correctness of this theory more comprehensible. It has already been mentioned that in many transverse sections of the albuminoid cylinders, unmixed with epithelial remains, the epithelium is found perfectly intact, and, though decreased in its normal thickness, nevertheless appears evenly colored, showing the absence of fatty metamorphosis. This fact has been advanced by *Weissgerber* and *Perls*,* for the purpose of showing the fallacy of the theory in question, but loses its significance if explained in another way. These investigators probably supposed that if the cylinders were a product of secretion from these cells, the latter should still contain a portion of this product, and accordingly present the same yellowish and glistening aspect as the cylinders. This condition of things, it is true, may be conjectured, and I presume does, in some instances, exist, and may sometimes also be observed, though its actual observation is by no means essential; for, having observed the deep color and glistening appearance on the cells of collecting tubules—in some instances on the entire epithelial ring—which had preserved their normal turgescient form, I am

* I. c.—*fuer das*, 1876.—Vol. I, p. 263.

inclined to regard this abnormal secretion as the initial stage of the degenerative process, followed, directly after the discharge of the secreted product, by the atrophy of the cells, before mentioned. It is, therefore, not essential that the cells surrounding a cylinder, representing the discharged product of their secretion, should in all cases still present the optical characters of this product; on the contrary, after the complete discharge of the secretion, they may not only resume their former appearance, but at the same time appear atrophied by the loss of material. But even if this supposition were wrong, there remain other probabilities of the cylinders being in truth a product of secretion; for it is not essential that this product should have come from those particular cells by which it is surrounded. On the contrary, it is more likely that it has been secreted from cells situated higher up, and, being at the time of secretion in a semi-fluid condition, has descended until arrested by the ensuing coagulation at another part of the tubule, in which the epithelial cells have not been similarly affected.

In some instances, as has been mentioned, the epithelium, generally surrounding the cylinder or infarction, has entirely disappeared, the latter being in direct contact with the basement-membrane. In these cases it may be presumed that these infarctions, consisting of epithelial remains saturated with the albuminoid matter, were probably formed in the place where they are found, while purely albuminoid cylinders are derived from cells situated higher up in the tubules.

It has already been noted that, in some instances, the minute vessels of the glomeruli were found filled with blood-corpuscles, showing that the congestion had extended throughout the whole capillary network of the interlobular vessels; generally, however, the vessels of the glomeruli were empty, and appeared normally colored; in a few instances they even appeared contracted, and the glomerulus diminished in size. In examining a considerable number of sections for the special purpose of discovering the peculiar condition of the epithelial cells above described, also on the layer of cells covering the vessels of the glomeruli, I only succeeded in observing in some parts of a few glomeruli the deep coloring and glistening appearance; besides this, I also met with

the segments of a few capsules, upon the inner surface of which an exudate, moderately colored by the carmine, could be detected. Another observation, however, quite as interesting, consisted in the presence of fat-globules, such as might be derived from a fatty infiltration, in a number of the cells of the covering epithelial layer of many glomeruli. These observations show that these cells also are prone to be affected in the same manner as those lining the uriniferous tubules.

As in the liver and stomach, so in the kidney, extravasations of hæmoglobin take place from the capillary vessels, to be absorbed by the epithelial cells of the uriniferous tubules. Almost in every section of yellow fever kidney, some traces of free hæmoglobin, recognized in carmine preparations by their peculiar brown color before mentioned, may be observed in any portions of the tubules. Sometimes the extravasation is observed in a considerable number of neighboring tubules. The pigmentation, however, is not confined to the formless hæmoglobin, for hæmatoidin in granular or crystalline form is also met with. Hæmatoidin crystals, especially, are frequently seen upon the inner surface of the epithelium, generally collected in small groups or masses of very minute crystals, though in one particular case I also observed numerous granules of hæmatoidin in the interior of a number of empty uriniferous tubules. The brown color which the epithelial cells assume by the absorption of the hæmoglobin, of course, differs in intensity in different localities, according to the quantity absorbed. These extravasations are not limited to kidneys extensively degenerated, but are equally observed in cases presenting only slight traces of the degenerative process.

As far as my experience extends, there is no product of inflammation found in the yellow fever kidney. Although I had, in every section examined, my attention directed to this point, I failed to discover any trace of such a product, either in the vicinity of the connective tissue capsule, or in the interstitial tissue. Therefore the destructive changes occurring during the course of yellow fever, in the parenchyma of the kidney, are the result of hyperæmia.

In conclusion, I may remark that no traces of bacteria, or other minute organisms, could be discovered in this organ. If such

organisms were really present in the kidney, as has been erroneously stated, we should most probably find them in the minute blood vessels, or in the interior of the uriniferous tubules. But not a single specimen, or colony, have I met with in these localities in the very numerous sections which I have examined. And if ever these organisms are met with in these places, a close investigation will certainly show that they were developed along with the decomposition of the body, or of the kidney itself, after its removal from the former. In one exceptional case only, I observed upon a section, mounted in Canada balsam, one or two granular patches, representing colonies of minute micrococci. A change of the focus, however, showed that they were not contained in the preparation, but were simply resting upon its surface, where they had been developed after the section had been cut, an accident which probably occurred in the staining fluid, or while the sections were kept in a mixture of alcohol and water, previous to the process of mounting.

THE SUPRA-RENAL BODIES.

These organs, of which the true function is, as yet, not known, are in fatal cases of yellow fever almost invariably found to have undergone certain pathological changes. To render a description of these changes more perspicuous, we may, as before in some other instances, briefly review their microscopical anatomy.

As in the kidney, so in the supra-renal bodies, a "cortical" and a medullary substance have been recognized—though here no secretory ducts exist, but both substances consist of minute blood vessels and cells, supported by a fine reticulated network of connective tissue. This network originates by certain septa, or processes, arising from the inner surface of the general capsule of the organ, whence it extends throughout the parenchyma, to be finally connected with the adventitia of the veins, which leave the organ through its hilus. The blood-vessels of the organ are very numerous, and represent branches of the aorta, and of the phrenic, cœliac, and renal arteries, which, after penetrating the capsule, subdivide into numerous minute branches, terminating in the capillaries of the cortical substance. Three distinct layers of cells are distinguished in this substance. The middle one of

these layers truly represents the bulk of the cortical substance, while the outer and inner ones only appear as narrow borders. The inner layer blends with the adjoining medullary substance. The cells, which are polygonal or round in form, occupy the space between the septa in the form of larger or smaller groups or columns. In the outer and inner layer, these groups, in accordance with the smaller and rounder meshes of the reticulum, are small and of a roundish form, while in the middle layer, in which the meshes are larger and much longer, the groups of cells assume the forms of columns. The medullary substance consists of a reticulum of very delicate connective tissue, with small meshes, filled up by small groups of polygonal or stellated cells, containing large nuclei. The capillaries, differing in their caliber in the different localities of the parenchyma, generally follow the septa and columns of the reticulated connective tissue, and, accordingly, the form and size of their meshes correspond with those of the meshes of the reticulum. Their caliber is of a medium size in the cortical substance, but in reaching the medullary substance, in which they represent a network with small meshes, they considerably enlarge in diameter, and many of them even present dilatations. In the latter network, the venous radicles take their origin. The nerves in the supra-renal bodies are very numerous; they are derived from the semi-lunar ganglion, the renal plexus, and the phrenic and pneumogastric nerves, and, after having entered the organ, are chiefly distributed to the medullary substance, forming plexuses in connection with which ganglion-cells have also been observed. The pathological changes observed in the supra-renal bodies resemble those taking place in other organs already described, consisting in the infiltration of fat and hæmoglobin, derived from the blood, and preceded by hyperæmia of the organ. In most cases, therefore, the examination of thin microscopical sections shows the capillaries filled with blood-corpuscles, especially those larger ones extending through the medullary substance, though in a number of cases I also observed the others, particularly those of the outer layer of the cortical substance, congested. The fat-globules, resulting from the fatty infiltration, are quite large, and are found in the cells of both substances, though perhaps to a greater extent in the cortical.

The infiltration or extravasation of hæmoglobin absorbed by the cells is greater and more general here than has been observed in any other organ. Almost in every case examined, it involved the whole medullary substance, from which it extended into the inner and middle layer, sometimes even into portions of the outer layer of the cortical substance. The degree of this pigmental infiltration is sufficiently great to be always distinguished by its brown color in sections of fresh specimens; in some cases, even, it appears dark brown. In sections, stained with carmine, the infiltrated cells exhibit the characteristic brown color of the extravasated hæmoglobin, already referred to. Besides the fatty and pigmental infiltration met with in every specimen examined, I moreover observed, in some cases, that a process of atrophy of the parenchymatous cells, particularly those of the medullary substance, had also been going on. In some cases, even, the atrophy was associated with softening of this substance, which presented a dark brown color; the softening process had given rise to a cavity in the organ. The atrophy appeared to be chiefly confined to the protoplasm of the cells, the nuclei remaining unaffected.

For the sake of comparison, I prepared a number of sections of supra-renal bodies taken from several cases representing other diseases than yellow fever, such as of the heart, liver, kidneys, and lungs. In these sections, a trace of pigmental infiltration was detected, but too insignificant to be in any way compared with that observed in yellow fever, as it was entirely limited to a very narrow yellowish brown stripe, representing the inner border of the cortical substance.

Although a number of pathological conditions and structural changes, such as hypertrophy, atrophy, fatty and amyloid degeneration, suppurative inflammation, carcinoma, tubercle, cysts, and even extensive hæmorrhages in the interior of the organ, have been observed to occur in the supra-renal bodies, I have thus far seen no record of an infiltration of hæmoglobin into the parenchyma in such a marked degree as I have observed to take place in yellow fever,—a phenomenon to which some special significance regarding the normal function of these organs might be attached, if it was not simultaneously observed in the liver, stomach, and kidney. For this reason, it must be regarded, as

in the other cases, depending upon the hyperæmic condition of the vessels, retarding the circulation of the blood through the latter. The fatty infiltration, also, is evidently due to the same general causes, from which it depends in the other organs already mentioned.

THE CEREBRO-SPINAL AXIS.

The Pia Mater.—In connection with the macroscopical examination of the organs after death, I stated that this membrane, as far as it extends over the brain, was almost invariably found in a state of hyperæmia, and that, in many cases, not only the veins, but also the arteries were found filled with blood. The microscopical examination reveals the same condition in the minute vessels, the arterioles, venules and capillaries. In examining, therefore, a small piece of pia mater, carefully removed from the cortex cerebri, these vessels, with only a few exceptions, are, in most instances, found more or less filled with blood corpuscles; in some of them, even, the corpuscles are crowded to such an extent as to have lost their original form by the mutual pressure which they had exerted upon each other. In removing the pia mater, the minute vessels entering the substance of the brain are, of course, torn, causing the blood corpuscles which they contain to escape. The stumps of these vessels, appearing like villi, and attached to the inner surface of the membrane, are usually found empty. Notwithstanding the intense hyperæmia which appears to have existed during life in many portions of the pia mater of the brain, I have, with one single exception, failed to detect any exudation cells in the vicinity of the vessels, or any increase in the connective tissue cells of the membrane, which might have indicated the pre-existence of inflammation,—a fact, which clearly shows that the cerebral phenomena observed at the bedside, are the results of a simple though severe hyperæmia. In the one exceptional case, the connective tissue cells had just commenced to undergo a division, resulting in a small group of three or four cells, or only nuclei, in the place of the old ones (Fig. 17). In some cases, I have observed that the walls of a number of arterioles, especially of those which are empty, or containing but few blood corpuscles, presented a corrugated or wrinkled appearance. As the wrinkles were confined to the

adventitia, they appear to have been caused by a contraction of the delicate muscular fibers, encircling these small vessels. If this be the true cause, the contraction must have occurred before death, or during the agony of death itself. But there is another pathological condition, which, in fully one half of the cases examined I observed to have taken place in these minute vessels, both arterioles and venules, and which consisted in a fatty degeneration of their nuclei, especially of those belonging to the adventitia. In most of these instances, the nuclei have disappeared, leaving a group of smaller or larger fat globules in their places. In other instances, an increase in the mere trace of protoplasm, found in connection with the nucleus in its normal condition had taken place, causing a thickening of the wall of the minute vessel, and giving rise to a capillary aneurism, or, by its proneness to degeneration, even to a final rupture followed by hæmorrhage. The small hæmorrhagic effusions, observed in some of these cases, may have been owing to this cause.

The arachnoid membrane, as before stated, is very frequently found not only opaque, but also thickened. This condition is caused by an exudation of a finely granular matter into the subarachnoid space, filling up, at the same time, the meshes of the connective tissue of the pia mater. The granules of which this substance is composed, are quite distinct, but pale, and measure about $\frac{1}{1200}$ mm. in diameter. In the pia mater of some cases, and mostly in the close vicinity of a vessel, or associated with a minute hæmorrhagic effusion, smaller or larger brownish looking granular masses of irregular form, are here and there observed. The granules of these masses are identical with those of the exudate before mentioned, though they appear more distinct, which is probably owing to the presence of free hæmoglobin, imparting the color to the mass. A number of similar, but colorless granular accumulations are moreover observed in the spaces between the vessels. It is quite probable that these accumulations represent minute portions of granular substance from the upper stratum of the cortex cerebri.

The hyperæmia of the pia mater is not confined to those portions of the membrane covering the cerebrum and the cerebellum, but, moreover, extends over the pons varolii and medulla oblon-

gata, frequently even as far as the cervical enlargement of the spinal marrow. In many cases, the congestion of the vessels of the pons and medulla is greater than that of the central vessels.

While in the cervical and dorsal regions, the pia mater of the spinal marrow is generally found free from congestion, its vessels are almost always found filled with blood corpuscles in the lumbar region. This fact can be sufficiently demonstrated in thin sections of spinal marrow from this region, in which the minute vessels of the membrane, with those that enter the spinal marrow, are observed filled with blood corpuscles.

The Substance of the Brain.—As it may well be asserted, that in yellow fever the safety of the patient almost entirely depends during the febrile stage, and after, upon the particular condition of the nervous system, especially the brain, an exact knowledge of the pathological changes taking place in this organ, are most important. And, judging from the traces of intense hyperæmia, met with in almost all fatal cases in the pia mater, as above described, we may well expect to find a similar condition in the substance of the brain. The pathological condition of this organ may be studied to the greatest advantage in large thin sections, passing through the whole extent of its different parts. The examination of a very great number of such sections revealed to me that, in almost every case examined, the hyperæmia extended from the pia mater *throughout* the substance of every portion of the brain. Not only the arterioles and venules were found filled with blood corpuscles, but also the capillaries. In most instances, the blood corpuscles are crowded in the arterioles and venules, and to such a degree as to have lost their normal form by their mutual pressure. In the pons varolii and medulla oblongata particularly, the vessels are invariably found filled with blood corpuscles, inducing me to regard these parts as special seats of the congestion. In some cases, I have even found opacity of the pia mater covering these localities. In those cases in which there was fatty degeneration of the arterioles and venules of the pia mater, this process is observed to have extended to the same minute vessels of the brain substance. In some cases, even, I observed traces of commencing degeneration in a considerable number of ganglion cells, especially on those of

the cortex cerebri, but also in the medulla oblongata. While in a normal ganglion-cell, namely, the double contour of the nucleus, and the outlines of its granules and those of the protoplasm of the body, generally seem distinct, they here appeared indistinct, and the whole ganglion cell presented a fatty luster. This condition is best observed in uncolored sections, examined in water or glycerine. In one case, I observed that though the outlines of these anatomical constituents of the ganglionic bodies of the cortex cerebri had been well preserved, their protoplasm had undergone atrophy, reducing their size. Notwithstanding the occurrence of extravasations of hæmoglobin in every other organ examined, the pia mater not excepted, as I have shown before, the substance of the brain appears to be remarkably free from this pigmentary infiltration; neither were in all the cases examined microscopically—about a dozen—capillary hæmorrhages met with.

As regards the *spinal marrow*, there are, aside from the congestion of the vessels of the pia mater above mentioned, no pathological changes observed in the nervous elements.

THE GANGLIA OF THE SYMPATHETIC NERVOUS SYSTEM.

The microscopical examinations which I made on these ganglia, embraced a very considerable number of sections, made of the *first thoracic*—*G. stellatum*—and the *semi-lunar* ganglia of six cases. In the majority of these cases, the minute blood vessels of these ganglia were found filled with blood corpuscles. In two cases, the ganglionic bodies of the first thoracic, as well as of the semi-lunar ganglion, had most obviously undergone degeneration. In the greater part of these ganglion cells, the nuclei had entirely disappeared (Fig. 16), leaving no other trace but their nucleoli, which appeared very distinct; in the rest, *very faint* outlines of the nuclei could be still observed. The bodies of all these ganglion cells presented an indistinct appearance, and were characterized by a peculiar fatty luster, even observed on specimens mounted in Canada balsam. As there were no fat globules observed in the places of the nuclei, it is difficult to account for their disappearance, unless it was caused by a process of atrophy, such as was observed to occur on the epithelial cells of the

uriniferous tubules. In a third case, I observed an abnormal accumulation of pigment in a considerable number of ganglion cells of the semi-lunar ganglion. In the three remaining cases, nothing specially abnormal could be detected on the ganglion cells.*

The preceding description of the pathological changes taking place in various organs and tissues in the course of yellow fever, is based upon very numerous and careful examinations, made during former epidemics, but particularly upon the abundant material which I collected during the epidemic of 1878. In the pursuit of these studies, portions of the organs were examined in their fresh condition directly after the autopsy, while the rest were put into a simple solution of bi-chromate of potassa, or into Mueller's fluid. A few days later, after the tissue had been slightly hardened, the examinations were repeated upon thin sections made by the free hand; but after they had attained the proper consistency, larger and perfect sections were made by the aid of the microtome. These, finally, were carefully prepared, stained, and mounted in Canada balsam or glycerin, in order to serve for the more thorough final studies.

The amount of material which I have in this manner examined and studied is very great; it embraces the various organs of twenty-three cases out of the thirty autopsies which I made during the epidemic of 1878, and those of an additional most interesting case in 1879. I have made more than two thousand thin sections, ranging from the larger ones, passing throughout an entire hemisphere of the cerebrum to the smaller of the sympathetic ganglia, or the mucous membrane of the stomach. The

* In the "Report of the Havana Yellow Fever Commission of the National Board of Health," it is stated that in the semi-lunar ganglion "connective tissue of new formation is met with to a greater extent than in the tissues heretofore described, but the nervous elements present no evidence of degeneration further than the cloudy swelling already described in other organs; while of the other portions of the nervous system which have not been examined microscopically, it may be said, that they present nothing abnormal to the naked eye."

The discrepancy existing between these statements and my own observations, I can only explain by presuming that the pathological expert of the commission took the very numerous nuclei, belonging to the capsules, and to the intricate extensive plexuses of sympathetic nervous fibrillæ connecting the ganglionic bodies, or to the bundles of sympathetic nerve fibers arising from them, for newly formed elements. As regards the report of the condition of the other portions of the nervous system, I could only infer that the type of yellow fever at Havana must differ from that observed at New Orleans.

sections of the brain are especially numerous, comprising all parts of the organ, and representing a considerable number of cases. About one thousand of these sections, taken from different organs, I have mounted in Canada balsam or glycerin, a large portion of them being in the possession of medical friends in New York, Philadelphia and Chicago, while the rest remained in New Orleans.

A word concerning the mounting and examination of these or similar specimens may be advisable here, as their successful study much depends upon the medium in which they are examined. Thus, though the mounting in Canada balsam is the most substantial and beautiful for colored sections of tissue, there are nevertheless certain disadvantages connected with this medium, and depending upon its high degree of refractibility. This is the case with tissues that have suffered fatty infiltration or degeneration, in which the fat globules, by the too great amount of light passing through them, are rendered quite indistinct, and difficult of recognition. For this reason, it is better to examine and mount such specimens in a less refractive medium, such as glycerin. In examining stained tissues, mounted in balsam, they should not be too brilliantly illuminated, as too much light will render indistinct the image of structure, though it shows well the image of color. It is more advantageous, therefore, to use the plane mirror and a small diaphragmatic opening for the sections mounted in balsam, while the glycerin sections may be examined with the concave mirror. I need hardly mention that examinations of this kind should only be made with the very best first-class objectives, as an inferior objective does not show the details in their true light.

[TO BE CONTINUED.]

THE Minnesota State Medical Society will hold its annual meeting at St. Paul, on Tuesday, June 20, 1881. The profession is respectfully invited to add to the interest and value of the meeting by being present. ALEX. J. STONE, M.D., *President*.

Clinical Reports.

ARTICLE V.

SURGICAL CLINIC OF PROF. EDMUND ANDREWS.

Litholapaxy.

It is doubtful whether Prof. Bigelow is justified in coining this new name for a mere modification of lithotrity. It is best to be very slow to inflict new technical terms upon the memory of students.

The word is compounded from the two Greek terms, *lithos*, a stone, and *lapaxis*, the action of purgative medicines on the bowels. Literally it would mean "stone-purging." The essence of the operation consists in removing the whole stone at one operation, instead of doing it little by little at several sittings. For this purpose Prof. Bigelow has devised a wash bottle and tubes far superior to those of Mr. Clover, and by the great size of his tubes immensely facilitates the evacuation of the fragments. He has also modified the lithotrite, so that it enters the bladder easier, crushes the stone faster, and is less liable to pinch the folds of the bladder, than the old instruments. Nevertheless his operation has hitherto found little favor among Chicago surgeons, but for my part, believing that small stones in adults ought generally to be removed by crushing rather than cutting, I am prepared to look favorably on Bigelow's plan in many cases, for I have had so much experience in the vexations of slow lithotrity, that I rejoice heartily in any improvement that can combine the safety of lithotrity with the promptness of lithotomy. The following cases occurred at the clinic during the past month :

Case 11,348.—This patient was admitted in excellent general

health, so that his system scarcely required any preparation. Ether having been administered, the large lithotrite of Bigelow was introduced, and the scale on the staff, after the stone was seized, showed it to have the diameter of two and a half centimeters, or about seven-eighths of an inch. In twenty-five minutes the stone was so completely crushed and washed out, that not the smallest particle could be found by a delicate searcher with the auscultating attachment. In order to introduce Bigelow's large tubes it became necessary to incise the meatus, when they entered without difficulty, and their great superiority over the smaller tubes of Mr. Clover was strikingly manifest. The crushed stone seemed to be sucked up and brought out with the sweep of a small maelstrom.

The patient, on awaking from his ether, declared he had not been so comfortable for months, and could hardly be kept in bed the rest of the day. The following morning he got up in spite of remonstrance, declared that he was perfectly cured and wanted to go home. I explained to him that it was necessary to stay and be sounded a few times to make sure of the completeness of the operation. This pacified him for a few hours, but in the afternoon he declared that he *knew* his cure was complete, and that he never felt better in his life; so, suiting the action to the word, he marched to the depot and took the cars for home.

This imprudent course caused no injury, so far as I can learn, but it is not to be recommended to others for imitation. The case, however, showed in a striking manner how much superior rapid lithotrity is in some instances to either slow lithotrity, or to lithotomy.

Case 11,367.—This patient entered with a stone which, when grasped by the lithotrite, measured eight millimeters in diameter, or about a quarter of an inch. Notwithstanding the small size of the calculus, the bladder was more sacculated and irritable than in the former case. The urethra was small, and even after incision of the meatus would not admit Bigelow's large tubes. As the patient could not stay for gradual dilatation, I proceeded by attaching Mr. Clover's tubes to Bigelow's energetic wash bottle. The inferiority of the Clover tubes was manifest, and though the crushing was readily effected, the operation lasted under ether

thirty-five minutes. In both patients Bigelow's splendid lithotrite showed its superiority to those of Sir Henry Thompson, and of Sir William Ferguson. It seizes easily, and with the least possible liability of grasping the folds of the bladder between the jaws.

The patient awakened and had no shock or chill. He was tractable, lay in bed a few days as ordered, and, after being resounded to determine the completeness of the operation, went home cured on the sixth day.

TURPENTINE TO PREVENT VOMITING UNDER ETHER.

There has been a statement going the rounds of the profession that a dram of spts. turpentine in each pound of ether will prevent anæsthetic vomiting. Hoping to derive advantage from this plan, I had the ether used at the clinic terebinthinated for a time, and a record of the results kept to compare with those of the non-terebinthinated article. The comparison resulted as follows:

Total number anæsthetized during the experiments, 102; patients taking pure ether, 31; number who vomited, 6; per cent. vomited, 19. Patients taking terebinthinated ether, 71; number who vomited, 21; per cent. vomited, 30.

It appears, therefore, that of those who took pure ether, only nineteen per cent. vomited, while of those who took the turpentine and ether, thirty per cent. met that accident. It is needless to say that the turpentine was ignominiously expelled from the clinic. No. 6, Sixteenth street, Chicago. E. A.

STUDY OF CAUSES OF CONSUMPTION.—Dr. Edward Playter, of Toronto, Ontario, has undertaken a systematic study of the causes of consumption. As an aid in this study he requests members of the profession having well-marked cases of consumption now under treatment to send him their address, in order that he may send a list of questions concerning the consumptive patient to which he wishes replies. A study based on a sufficient number of replies must yield valuable results.

Society Reports.

ARTICLE VI.

ILLINOIS STATE MEDICAL SOCIETY. Thirty-first Annual Meeting held May 17, 18 and 19, 1881, in the Methodist Church Block, Chicago. Dr. George Wheeler Jones, of Danville, President, in the chair; Dr. S. J. Jones, of Chicago, Permanent Secretary.

FIRST DAY—MAY 17.

This was one of the largest meetings ever held by the Society, whose membership numbered 114, but the audience was reinforced by an unusually large number of visitors from the city, and this and other states. The

PRESIDENT'S ADDRESS

was decidedly conservative in character, and strong in its denunciation of the many abuses which had crept into the profession at large. Effectual medical legislation seemed an almost impossible achievement, and he denounced the present medical training as inadequate to the exigences of modern practitioners of medicine, and advised a graded system of education.

The report of the Committee on Practice of Medicine was read by the Chairman, Dr. E. P. Cook, of Mendota. It covered the time from May, 1880, to the date of this meeting, and consisted of reports from the various parts of the State. It was thought that no important phase of any epidemic had been overlooked, but there was much difficulty in getting the particulars of each, and a more efficient form of correspondence ought to be established between the various members. The appointment of correspondents from the local societies through Illinois might answer this purpose. Nothing unusual had happened until October,

1880, except a slight increase in malarial diseases, but from that time these and many other zymotic diseases had prevailed to quite an extent. The protracted winter from which we had just emerged was doubtless a powerful factor in their causation. The southern part of the State had been more favored. The measles prevailed in every county. Scarlatina and diphtheria prevailed in an epidemic form in some of the central and northern counties. Cerebro-spinal fever complicated some cases of measles. Small-pox had extended from Chicago to most parts of the State. Pneumonia had also been unusually frequent, and an epidemic of dysentery had been observed. The mortality from whooping-cough, and that from erysipelas had been greater than usual. Winter cholera had prevailed in Chicago; it was so mild as to escape the observation of many. A few cases of trichinosis had been reported, but probably many more had been overlooked, as was generally the case.

Report of Dr. Charles T. Parkes, of Chicago, Chairman of the *Committee on Surgery*.

TREATMENT OF WOUNDS.

Free drainage, close coaptation, and perfect rest are requisites. The first was obtained by the use of rubber tubes, or what was better, absorbable tubes made of chicken bones. Silk-worm guts form an advantageous substitute to horse hair in capillary drainage. To insure coaptation, the cat-gut ligature was very good, but as it was soon absorbed it might give rise to secondary hæmorrhage, and torsion was still preferred in some hospitals in the arrest of hæmorrhage. Cat-gut soaked fourteen days in one part of chromic acid to thirty of glycerine was so hardened as not to be so readily absorbed. Silk-worm guts were a good substitute. Rest of a wounded limb should be secured by the use of splints in some cases.

Listerism was now adopted by most of the younger surgeons, and when the spray was dispensed with, the washes with carbolic acid solutions were used. Lister's dressing also contributed to insure perfect rest of the wounded parts. The presence of bacteria in suppurating wounds had been often detected, and when injected into healthy tissues gave rise to abscesses, but under the use of the spray, these bacteria had disappeared from wounds.

The dressings should be renewed often enough to prevent any irritation to the parts from retained fluids.

VARICOCELE.

The treatment of this disease was very unsatisfactory, frequent cases of death or of permanent injury occurred, and Dr. R. G. Bogue had made quite a departure from the common treatment, in opening the *scrotum* antiseptically, ligating a part of the veins at a time with cat-gut, and closing the wounds. This had given most satisfactory results, and could be applied also to varicose veins in any part of the body.

NERVE RESECTION AND STRETCHING.

The Chairman had made an efficient resection of the ulnar nerve in two instances, and sensation and motion were re-established after the extremities of the nerve had been pared and brought together with cat-gut ligatures. Nerve stretching was considered a favorable operation in neuralgias, tetanus, epilepsy, locomotor ataxia, and other diseases. Experiments to ascertain the tension of nerve trunks in men were made on recent subjects, with the following results: Eighty pounds break the ulnar nerve; 57, the median; 18, the radial; 61, the brachial; $7\frac{1}{2}$, the facial; 200, the great sciatic; 92, the cauda equina. Dr. Parkes thought he had been the first to institute such experiments.

A case of non-union after a compound fracture of the tibia in an old man, had comè under the observation of Dr. D. S. Booth, of Sparta. The foot was useless and amputation seemed proper.

Dr. W. A. Byrd supplemented the report on Surgery with some very interesting cases. In one he had trephined into the frontal sinuses of a patient, and removed some decayed bone which had caused a supra-orbital neuralgia of fifteen years duration. He had made tracheotomy without a tube, replacing it by wires. He related a new method of nerve section in facial neuralgia, and exhibited some surgical instruments remarkable for their simplicity, cheapness and usefulness.

Committee on Obstetrics.—Dr. H. Webster Jones, Chairman, read the report of that committee, and said that the accoucheur should inspire confidence, he should palliate suffering, supplement nature, and attend to any complication. His hand was the best uterine dilator. Forceps should not be used unless strongly indi-

cated. Long delay in labor was a common cause of inflammation and blood poisoning. The delivery of the placenta should be facilitated by placing one hand over the uterus externally. A careful examination should be made six weeks after parturition to satisfy the accoucheur as to the integrity of the various organs of generation.

Dr. De Laskie Miller laid great stress on the necessity for the accoucheur to be learned and skillful. He gave some rules for dilating the os with the fingers and tearing any adhesion of the membranes to the uterus. He said that rupture of the perineum could be avoided if the attendant had a proper knowledge of the anatomy of the parts, and made sufficient support.

Dr. J. W. Dora read the report of the *Committee on Gynecology*.

He complained that this is the only specialty which was not practically taught to students. He hoped for the time when false modesty would no longer interfere with clinical demonstrations. He denounced several modes of treatment which had been fashionable in their days and caused many bad results. His treatment of obstructive dysmenorrhœa consisted in making an incision and introducing sponge tents dipped in a carbolic acid solution. It needed to be used repeatedly, but it would generally prove successful. He gave rules for the treatment of rupture of the perineum.

Dr. Ellen A. Ingersoll read a supplementary paper on *Dysmenorrhœa*.

It was often caused by displacement, but the majority of cases could be referred to a congestive condition of the uterus. It was liable to occur soon after the age of puberty, and to disappear after repeated pregnancies. The general health was often below par. The function of the liver in excreting cholesterine, the detritus of nerve substance, was ill-performed in those cases, and constipation was a frequent accompaniment. Mercurials were especially suited to these. Alteratives, tonics, and hygienic measures were beneficial. For an immediate relief German chamomile, aromatic spirits of ammonia and viburnum were as good as any other remedy. The introduction of a sponge tent a few days before the return of the menses prevented dysmenorrhœa.

SECOND DAY—MAY 18.

Dr. F. C. Hotz, of Chicago, read the report on *Ophthalmology and Otology*.

Overdoses of quinine had produced blindness as well as deafness. In a case of keratitis, quinine had brought relief after local treatment had proven useless. Homatropine was an efficient dilator of the pupil, and its effects soon passed away. The report was unfavorable to the audiphone. Dr. Hotz gave the particulars of a case of malarial otitis which subsided after the use of quinine in scruple doses. Enucleation of the eye-ball could not be replaced by section of the nerves, as these would unite soon and sympathetic inflammation would continue.

Committee on Dermatology.—Dr. W. J. Maynard, of Chicago, read the report. His subject was ring-worm of the scalp. Its proportion to other diseases was one to thirty. Its diagnosis required time and skill, and so its treatment. It was quite liable to cause baldness. Ring-worm of the body was a common affection, but it passed unobserved. The type of the disease was lost after some time, and would readily be mistaken for eczema. The cause of the disease was a microscopic organism, which gave rise to the same in some of the domestic animals. The best treatment consisted in mercurials, sulphurous acid, iodine and carbolic acid. Epilation was necessary in many cases, and this was done with large forceps. Iron, arsenic, iodide of potassium were given internally with cod-liver oil.

Dr. C. T. Reber, of Shelbyville, reported two cases of submucous fibroids complicating pregnancy. There had been abundant hæmorrhage in both cases, but they had reached full term.

Dr. W. S. Caldwell, of Freeport, read a paper on the management of the after-birth in abortion. No great amount of hæmorrhage should be allowed, but the hand or the fingers should be introduced and the placenta removed. He did not commend the use of placental forceps, nor the administration of ergot. He had used the tampon and laminaria tents successfully in some cases.

Dr. E. Fletcher Ingals, of Chicago, read a valuable report on *Laryngeal Tumors*, which appears in full in this number of THE CHICAGO MEDICAL JOURNAL AND EXAMINER.

Drs. Truesdell and Whitmire recommended the use of the carbolic acid inhaler in chronic laryngitis and other affections of the air passages.

ANIMAL HEAT.

Dr. J. H. Hollister, Chicago, read an able paper on that subject. Heat was produced by the vital activity of the animal as much as by chemical changes taking place in the tissues. There was more heat generated in health, but its retention in disease gave rise to fever. He believed there were vaso-dilator nerves, whose primary center was situated in the medulla oblongata, which some medicines influenced, thereby diminishing heat. He also enumerated the various results of a high temperature. Withdrawing food, applying cold to the surface of the body, and administering some cardiac sedatives, reduced the temperature in fever. The cold pack, sponge bath, and ice internally, were especially recommended.

Dr. Hill, of Bloomington, exhibited an instrument invented by Dr. Russell, of Bushnell. It resembled a trocar and canula and was to be introduced into the trachea to replace tracheotomy.

Dr. Maxwell read a paper on *intra-capsular fracture*, and reported what seemed to be two cases of that injury in his own practice. He had made extension in the direction of the limb, and he had made extension transversely. He had also elevated the bed at the foot and on one side. The treatment had also been successful in both cases, and union was perfect.

SPECIALISM IN MEDICINE.

Dr. A. Reeves Jackson, of Chicago, said, in an able paper with the above title, that specialism was nothing new. That there were physicians in ancient Egypt who made a specialty of the treatment of the liver, the skin, the head, and the toe-nails. Multiplicity of professorships and a lack of medical education were the chief causes of specialisms. The consciousness of knowing very little of general practical medicine leads the graduate to work a specialty for himself. He remonstrated against the theoretical teachings of the various colleges, and said that medicine was chiefly an art which could be learned through practice only.

The advantage of specialism was that by concentrating it increased one's energy in a certain direction; that all great men had been specialists, and that it was for such a division of labor that medicine had progressed so rapidly of late. He called professional quacks those that studied a special branch before they had been general practitioners. These would be especially liable to refer all diseases to one organ in particular. His conclusion was that specialism reduced the number of general practitioners without diminishing the practice of the latter, and specialists relied on the profession for its support, but they must have been good common physicians before entering the field of specialism.

Dr. Earle read an article on rötheln, an abstract of which appeared in the last number of THE JOURNAL AND EXAMINER.

Dr. Roswell Park reported 100 cases of the same disease which had come under his observation at the Orphan Asylum.

THE LEGAL REGULATION OF THE PRACTICE OF MEDICINE.

Dr. M. A. McClelland, of Knoxville, had prepared a report on the above subject, which was read by the Secretary. It was a statement of the various attempts at making efficient legal regulations, with as many failures.

INFECTIOUSNESS OF TUBERCULOSIS.

Dr. H. Gradle summed up the late experiments made to ascertain the contagiousness of tuberculosis in rabbits. There was no doubt as to the relationship between scrofula, tuberculosis, and fungous arthritis. The incubation of the disease in rabbits was twenty days. Tuberculosis is probably often transmitted through the breath of the patient. A solution of tuberculous matter in a spray inoculated the disease in dogs in the space of three weeks. In all probability the virus was a micro-organism.

Dr. E. F. Ingals said that it was a matter of surprise to him that the relations of bacteria to tuberculosis had not yet been determined. He had no doubt as to the contagiousness of tuberculosis, but he thought another decade would pass before the character of this contagion could be ascertained.

USES AND ABUSES OF ALCOHOL.

Dr. E. Ingals read a report made up of the opinions of most of the prominent members of the Society. Most of them agreed

that alcohol should never be used except as a remedy. That in some cases it was one of the best articles in the Pharmacopœia, and a good stimulant. It was a narcotic poison and an anæsthetic in large doses. Drunkenness was a vice of advanced civilization, which should be combatted by the example and the principles of the medical profession.

THIRD DAY—MAY 19.

The first paper in order was one on Chian Turpentine, by Dr. E. Andrews, of Chicago. This remedy had proved useless in the treatment of any variety of cancer. A great many cases of cancer of the womb, of the breasts, etc., all of them following the usual course of the disease, were related, and the conclusion was reached that Chian turpentine did not deserve a better place than any other remedy in the therapeutics of cancer.

EPILATION.

Dr. Plym. S. Hayes, of Chicago, read an interesting paper on this subject. He claimed the best depilatory was electrolysis; which he performed thus: A needle was attached to the negative pole and plunged into the hair follicle, the hair being held by forceps; the other pole was placed near the spot, and the current destroyed the hair follicle. It required little traction to pull the hair, which could never grow again. This process was not painful, the electrolysis caused some anæsthesia of the parts. It seldom needed to be repeated.

ANATOMY OF THE SHEATHS OF THE PALMAR TENDONS.

Dr. Roswell Park read a contribution with this title, an abstract of which was printed in the last number of THE JOURNAL AND EXAMINER.

LISTERISM AND CARBOLIC ACID.

Dr. C. Truesdell read a report largely composed of the views of many surgeons on this subject. He reported a case of mummification of the finger from pure carbolic acid. It had been painless. He thought this remedy serviceable, especially on account of its antiphlogistic properties, and considered it the best local application to wounds and ulcers. Dr. J. P. Johnson, of Peoria, had used phenol with as good results as carbolic acid. He was especially

gratified with the use of carbolic acid as a cauterizing agent. It possessed the great advantage over all other acids that it is painless in its application. He had cured ulcers in this manner, and had successfully treated endometritis by the application of carbolic acid to the inside of the uterus. He was in the habit of applying it with a stick fashioned as desired.

The Committee on State Register had been satisfied that the Register of the State Board of Health entirely fulfilled the wants of the Society. The committee was discharged.

The Treasurer's report showed a very prosperous condition of affairs.

The *Committee on Necrology* reported the death of Dr. F. H. Davis, son of N. S. Davis, of Chicago.

A resolution was passed providing that members who had paid their annual dues during fifteen years should not be suspended for non-payment of the same in the future, but that they would not be entitled to the Annual Transactions.

A paper on "Drainage of Cavities in the Lungs," by Drs. C. Fenger and J. H. Hollister, of Chicago, was presented to the Society, and referred to the Committee on Publication, with all the other papers read before this meeting.

ELECTION OF OFFICERS FOR THE PRESENT YEAR.

President—Dr. Robert Boal, Peoria.

First Vice President—Dr. A. T. Darrah, Tolono.

Second Vice President—Dr. Ellen A. Ingersoll, Canton.

Treasurer—Dr. J. H. Hollister, Chicago.

Secretary—Dr. W. A. Byrd, Quincy.

Committee of Arrangements—Drs. Jas. Robbins, J. T. Wilson, F. Drude, J. A. Wagner, M. F. Bassett.

STANDING COMMITTEES.

Practical Medicine—Dr. W. D. Ensign, Rutland, Chairman ; Dr. John Wright, Clinton ; Dr. H. T. Godfrey, Galena.

Surgery—Dr. E. W. Lee, Chicago, Chairman ; Dr. D. W. Graham, Chicago ; Dr. J. P. Mathews, Carlinville.

Obstetrics—Dr. J. B. Davison, Malone, Chairman ; Dr. A. C. Corr, Carlinville ; Dr. J. Y. Campbell, Paxton.

Gynæcology—Dr. E. W. Jenks, Chicago, Chairman; Dr. G. W. Nesbitt, Sycamore; Dr. F. Cole, El Paso.

Ophthalmology and Otology—Dr. A. E. Prince, Jacksonville, Chairman; Dr. W. T. Montgomery, Chicago; Dr. C. Chenoweth, Decatur.

Drugs and Medicines—Dr. Joseph Robbins, Quincy, Chairman; Dr. T. J. Pitner, Jacksonville; Dr. L. L. Silverthorne, Charleston.

Næcrology—Dr. E. Ingals, Chicago, Chairman; Dr. W. Hill, Bloomington; Dr. W. West, Belleville.

SPECIAL COMMITTEES.

Relations Between Syphilitic and Non-Syphilitic Lesions of the Skin—Dr. James Nevins Hyde, Chicago.

On the Management of Diseases of Children—Dr. W. O. Mendenhall, Georgetown.

On Medical Legislation—Dr. W. A. McClellan, Knoxville, Chairman; Dr. D. S. Booth, Sparta; Dr. W. Hill, Bloomington; Dr. C. Truesdell, Rock Island; Dr. E. Ingals, Chicago.

On Diseases of the Nervous System—Dr. S. M. Wylie, Paxton.

MEMBERS OF JUDICIAL COUNCIL.

Dr. C. Truesdell, Rock Island; Dr. C. Goodbrake, Clinton; Dr. A. K. Van Horne, Jerseyville.

The next annual meeting will take place at Quincy.

Delegates to the American Medical Association, to meet at St. Paul in June, 1882.—Drs. J. H. Hollister, Chicago; W. T. Kirk, Atlanta; W. T. Montgomery, Chicago; G. W. Jones, Danville; E. P. Cook, Mendota; A. Hard, Aurora; R. Boal, Peoria; G. W. Nesbitt, Sycamore; Sarah H. Stevenson, Chicago; J. M. Cowan, Hennepin, W. R. Shinn, Fielding; W. M. Call, Princeton; Catharine B. Slater, Aurora; J. P. Johnson, Peoria; John R. Livingood, Rossville; J. S. Whitmire, Metamora; S. T. Hurst, Greenview; Ellen A. Ingersoll, Canton; D. Lichty, Rochelle; D. E. Foote, Belvidere; D. S. Jenks, Plano; R. G. Bogue, Chicago; T. D. Washburn, Hillsboro; C. Truesdell, Rock Island; A. H. Kinnear, Metamora; David Prince, Jacksonville; E. W. Jenks, Chicago; W. West, Belle-

ville; H. P. Newman, Chicago; D. S. Booth, Sparta; W. H. Fitch, Rockford; Thomas Galt, Rock Island; W. L. Ransom, Roscoe; J. W. Dora, Mattoon.

To the Wisconsin State Medical Society—Dr. Roswell Park, Chicago.

To the Indiana State Medical Society—W. O. Mendenhall, Georgetown; W. M. Chambers, Charleston.

To the Ohio State Medical Society—J. R. Corbus, La Salle; Daniel Lichty, Rochelle.

To the Michigan State Medical Society—J. H. Hollister, Chicago; E. W. Jenks, Chicago.

To the Kentucky State Medical Society—M. W. Hurst, Sweet Water.

To the Missouri State Medical Society—H. W. Morehouse, Danville.

To the Iowa State Medical Society—M. A. McClellan, Knoxville; S. L. Plummer, Rock Island.

To the Minnesota State Medical Society—B. F. Crummer, Warren.

To the International Medical Congress, to meet in London in August, next—Drs. R. N. Isham, Chicago; De Laskie Miller, Chicago; S. J. Jones, Chicago; W. A. Haskell, Alton; David Prince, Jacksonville.

ARTICLE VII.

WEST CHICAGO MEDICAL SOCIETY.

Dr. H. M. Lyman, President, read a paper on the

IDENTITY OF SMALL-POX AND COW-POX.

Soon after Jenner's discovery, a Mr. Ceely of England, experimented in communicating small-pox to cows, and after a few re-vaccinations, obtained common vaccine virus. It seems these experiments were ignored by most physicians of the present day. However, the late investigations of Pasteur with chicken cholera threw more light on the subject. Experiments similar to Ceely's had been made in Berlin in 1802. It was remarkable that cow-pox had become so rare since those days. It was not a primary

disease in cows, and must have originated from variolous milkers' hands. No disease had so many forms with different degrees of intensity as variola.

The present theory was the only one which could explain the immunity conferred by vaccinia, for a disease never protected from another disease. Thus anthrax vaccination could be accounted for as well. And in cases of emergency, he advocated originating new vaccine virus by inoculating cows with small-pox. The first vaccine thus obtained would almost have the character of variola, and should be first inoculated in another cow to investigate its intensity. It would be more likely to produce vaccinia with an eruption.

In Jenner's time, vaccination was thought to be a protective for life, and the statistics furnished very few cases of small-pox after vaccination, and obviously during the following seven years. That vaccine virus had partly lost its efficacy in England was a fact, and a time could be calculated when it would become quite innocuous if not renewed. The virus used in this country is of a more recent origin. The mortality from small-pox was less before Jenner's time than it is now. For through a series of ages small-pox, by perpetuating itself in man, had become partly attenuated (the fittest having survived), those whose systems had been least able to resist having died, and a sort of tolerance had been conferred through hereditary descent.

It was probable that variola had a *habitat* in China, or Central Asia, whence most epidemics seemed to have arisen. According to this theory, epidemics would stop after many had had the disease, and many others had obtained a sort of immunity from it through exposure to the epidemic influence. Dr. Lyman's paper is printed in full in this number of the JOURNAL.

DISCUSSION.

Dr. Norman Bridge, referred to Dr. Meyer's experiment in this country, by which an epidemic of small-pox had been started from the use of virus originated *de novo* by inoculating a cow with small-pox matter.

Dr. Lyman thought that would confirm the received theory, for a first culture would attenuate the disease only to some degree, and inoculation should have been continued in a series of cows.

Dr. Bridge believed that the first inoculations with vaccine virus had been as mild as later ones; while in epidemics, small-pox began severely in every new center.

Dr. Lyman said that epidemics generally developed in communities which had not seen the disease for a long period, and that a new culture was most virulent in fresh soil.

Dr. Bridge said every person was a fresh soil for the disease, but Dr. Lyman believed that exposure to the general influence of the epidemic seemed to mitigate their predisposition.

Dr. E. W. Lee partly explained the cessation of epidemics by an increased vigilance on the part of the people who soon became alarmed and established quarantines.

Dr. Bridge wished to know how to reconcile hereditary tolerance with small-pox inoculations so extensively used in the British Isles. These generally had the disease in a mild form, and in reckoning them with other cases of small-pox, would give this low percentage of death quoted by Dr. Lyman.

Dr. W. E. Clark remembered the time when small-pox was inoculated to insure immunity afterward; his parents had submitted to the disease. Not more than three or four per cent. died.

Dr. W. J. Maynard said contagious impetigo was liable to follow vaccination; he had seen a few cases.

Some of the members had observed that disease in a few cases, and others reported a few cases of death from vaccination with virus kept for some time, or carried in the coat pocket. It seemed the virus had been thus altered and acted somewhat like septicæmic matter.

ARTICLE VIII.

CHICAGO MEDICAL SOCIETY.

Stated meeting, May 15, 1881. Dr. E. Ingals, President, in the chair.

The paper of the evening, on

NUMERATION AND MEASUREMENT,

was read by Dr. Weller. He said that the idea upon which the Decimal System was founded was as old as written history; a

Roman scale, constructed as a lever with unequal arms, and graduated in decimals, had been exhumed from the ruins of Pompeii, while unmistakable evidences were found that the decimal scale was applied to measures of length and weight in Egypt in the days of the Pharaohs, in Chaldea, and when the Sanscrit was the popular language of the Hindoos. Ingenious mathematicians have constructed systems based on different radices—four, six, eight, twelve, sixteen, twenty-four and sixty-four. Charles XII of Sweden, constructed one based on twelve. Emmanuel Swedenborg, one on sixty four; but, “man, to whose nature imperfection clings as a part of it, cannot think a perfect thought, much less construct a perfect system.”

He now proposed a system based on the radix eight, which Dr. Taylor, of Philadelphia had discussed before the Pharmaceutical Association in 1859. This new system needed new characters and a new nomenclature. The first eight numbers would be *un*, *du*, *te*, *fo*, *pa*, *si*, *he*, *ok*. Zero would become *ze*. *Unok* or *nok* would be nine; *nokun*, eleven; *dok*, sixteen, *fok*, thirty-two, etc. A letter added to and preceding one of the numbers becomes a multiplicator, placed after it, it means addition. He did not believe the unit of the decimal system had any advantage over other measures of length, and said that the avoirdupois pound should be retained in this country; but this he proposed to divide according to his new system.

Followed a discussion of the paper by Drs. Whitmire and Ch. Kruzemmark, the latter had adopted the decimal system and was well pleased with it.

MATERNAL IMPRESSIONS.

Dr. H. D. Valin reported a case of that nature. He had lately delivered a woman of a big boy, and found the child's nose flattened on the right side. The mother said that when she was about three and a half months pregnant, she had entered a meat market where she met a man whose nose had been eaten away by a cancer. It made a strong impression upon her, and there was no doubt in her mind as to its having caused the child's nose to assume such deformity.

Dr. Weller had met a wonderful case of that nature. When three and a half months pregnant, a woman had fallen down a

ladder, and after delivery at full term, it was found that the baby's right leg had been broken below the knee, and that the fragments of the bones had united with a good deal of deformity.

Dr. J. S. Whitmire had seen a case of congenital fracture of the femur, following a tedious labor, but he had not been able to ascertain how the bone had been broken.

Dr. Weller related another case of maternal impression, in which the mother asked him before she had seen her baby, whether it had a white spot on the left eye? She had discovered an artificial eye from the manner in which the light was reflected from it, when in her third month of pregnancy. The child had such a mark and has it yet. He is an employè of the North-Western Railway Co.

Dr. R. A. Leonard said he had in his museum a fœtus whose features resemble very much those of a beggar on State street who had come under the mother's observation. Fœtus was acephalic and remarkable in many respects.

Dr. Jas. E. Mead was elected a member, and the name of W. H. Curtis proposed.

ANNUAL death rate per 1,000 in the chief cities of the United States for the year 1880 :

	Population.	Total No. of deaths.	Annual death-rate per 1000.
Boston.....	362,535	8,634	23.5
Providence.....	104,850	2,098	20
New York.....	1,206,577	32,245	26.7
Brooklyn.....	566,689	13,576	24
Philadelphia.....	846,980	17,701	20.9
Baltimore.....	332,190	8,216	24.7
District of Columbia.....	180,000	4,122	22.9
New Orleans.....	216,140	5,631	24.2
Louisville.....	123,625	2,618	21.2
Cincinnati.....	255,708	5,331	20.9
Cleveland.....	160,140	3,260	20.4
Chicago.....	503,304	10,469	20.8
Milwaukee.....	115,578	2,891	21.5
St. Louis.....	350,522	6,725	19.2
San Francisco.....	233,956	4,518	19.3

—National Board of Health Bulletin.

Domestic Correspondence.

ARTICLE IX.

COMPETITION.

MESSRS. EDITORS:—I indite this article with the hope of calling forth a similar comment from others of our profession. The subject in hand is of vital interest to all physicians and surgeons practicing their calling outside of the classic walls of their medical *alma mater*. I do not wish to cast a stone at any particular college or medical uncle who took our cash for wise instructions, dignifiedly imparted, when we of the common herd were in the embryo state. We always will hold them in remembrance dear, and as long as the learned and skilled professor will remain true to his office of imparting medical lore and absorbing legitimate clinical material, then, so far, I say amen. But, for a man occupying any chair at the shrine where all budding M.D.'s for the stipulated time, pay devotion and cash, for him to descend and compete with his own and the fledglings from other institutions, who, after years of patient study and constant practice, have ripened into able surgeons—this fact is what prompts this article. My revered father and my honored preceptors taught me to heed the established code of ethics, and, until I began active practice, I always supposed that the fountain head of professional ethics was at our medical schools; but when I speak for myself I know personally, that I only echo the same experience for others of the country laity. For example, and to illustrate the workings of competition from high sources backed by the high prestige of a special chair in an established college: I was in the office of Dr. —, in a neighboring town, where a patient presented himself for examination. Case, necrosis of the os-humeri, requiring surgical interference. The doctor was skilled in that branch of surgery, perfectly competent to operate; patient well able to pay proper fee. Patient inquired amount of same,

and he himself set a day for the operation, but failed to appear. I made it my business to ascertain the reason. Found that he had become alarmed, took flight to the city, had consulted the professor, who concurred in diagnosis and was informed of amount of surgeon's reasonable fee for operation at home. Professor deliberately competed and took in the lucre. I was consulted by a gentleman with a diseased optic, case for immediate enucleation. Advised operation, and stated reasons so that patient could understand. He appreciated the importance of the information and set a near day for the operation, but failed to appear. I bided my time, and discovered that some one had advised him *to go to college where they would take it out for nothing*. This patient was wealthy and my fee was moderate. He acknowledged to me afterward, that he had expected to save fee by free operation at the college clinic, but decided not to make a show of himself, and had determined to return to me. When the chair of ophthalmology learned this fact, it bent down to inquire the amount of my fee, deliberately competed, and the lucre was kept out of home circulation.

I could enumerate over twenty cases of recent and more active competition than the above, for I spared no time or expense in order to get at the facts and satisfy myself truly, why consulting cases seldom return from their trip unplucked after crossing the spider's portals. In this manner does the country doctor's *honorarium* become a *non est*. During my last visit to the city one source of amusement was to notice the workings of the professional man from high to low stations, and I find the greed for coin overtops all points that can be strained. I hope all of our able country physicians and surgeons will awake to these facts and consult their own interests by more unanimity of feeling and recognition of each other's worth, and assist each other to combat this tendency to absorb our legitimate cases, especially those inclined to flee to the cavernous maw of the delusive college clinic. Any physician who has had experience at a college hospital where he could get an idea of each professor's private income from pay cases, is aware that a major portion of such income is derived from an unfailing supply of patients from the country. Now too often is it a fact that cases are referred to

professor so and so, in preference to turning the same over to our competitor at home who is perfectly competent to properly attend the same, and who would give closer and more undivided care to the case than the patient could possibly obtain at the professor's crowded clinic. Surely the medical millennium would be at hand if this *odium medicum* could be overcome. At the founding of a medical college one principal card to induce students to attend is the charitable free clinics, and a grand good school it is. The clinic is advertised to be supported by the State and materialized by the abundant paupers. This is the correct and proper source to draw from, for it does not interfere with the income of the special physician or surgeon either of the city or country. But when the free clinic is open to the whole and adjoining States regardless of the ability to pay, of many applicants, then do we gnash our teeth over the loss of that case and the needed fee; also, not to speak of the absorption of the pluckings by the professor at his elegant private office where the sufferer is impressed with the belief that therein is condensed the quintessence and *ultima thule* of medical skill.

Do you not see my dear country doctors wherein lie our grievances? We set our professional hook baited with half a life's hard study; little cases come and nibble, then a big bite. We have the prize half landed (in our mind), when lo, the shark of the college clinic gobbles it up! Let us reform and protect ourselves. If Dr. A. does not feel at home with a case, send it to Dr. B. across the street, who is just the man for the particular case. Do this in order to uphold the dignity and honor of our home and country practice. Please do not consign a patient to the cramped wards of a hospital when they can secure as good services at home, with the benefit of country air and wholesome diet. Every community is blest with a competent physician or surgeon, whose door cheerfully opens at the knock of even the suffering poor. But I had better close this communication for I may have offended some one; if so, I beg pardon, for I have only jotted down a few of my observations on this internal competition and jealousy among our profession, and think it time reform should set in. This article may not meet the approval of every reader, but, *damnant quod non intelligunt*.

Goshen, Indiana.

DR. J. W. LAMBERT.

Reviews and Book Notices.

ARTICLE X.—A MANUAL ON DISEASES OF THE EYE AND EAR; FOR THE USE OF STUDENTS AND PRACTITIONERS. By W. F. MITTENDORF, M.D., Surgeon to the New York Eye and Ear Infirmary, etc., etc. New York: G. P. Putnam's Sons, 1881.

This book, it seems to us, fulfills the author's promises, as set forth in the preface, better than any other manual of the same character of which we have knowledge.

Addressing itself to medical students and practitioners, and intended to convey to them practical advice and useful information, it is written in plain language and concise form. What we approve of most particularly is the exclusion from its pages of all questions which are still unsettled and under discussion; for their introduction into text books for the beginner bewilders his mind and often causes him to abandon the study entirely.

The ophthalmological part of the work deserves our special and unqualified approval. Nothing of importance is omitted. The symptoms of the diseases delineated in clear and graphic descriptions, the directions for their treatment betray the sound judgment of the expert, and in the prescriptions in which the pages abound, we find those remedies which are valued as useful and efficient by the majority of oculists.

We are very glad to find the author has not joined the ranks of the opposition to the use of silver nitrate in the treatment of blennorrhœal ophthalmia. Our experience, however, would not allow us to subscribe to his statement (p. 92) that "it is not necessary to use a stronger application than a five-grain solution of nitrate of silver once every day." We had strong reasons in many cases to attribute the ultimate success in our treatment to the use of much stronger solutions after that of five grains had been tried faithfully but unsuccessfully.

The colored plates introduced to facilitate the recognition of diseases will, in few instances, fail to accomplish this intention of the author. For instance, fig. 8, marked "conjunctivitis," looks by all means more like the picture of incipient iritis; for its most prominent features are an intense pericorneal redness and a slight irregularity of the pupil. In fig. 10, marked "granular conjunctivitis," the ocular conjunctiva is given the brownish tint it occasionally attains after long continued use of silver (Argyrosis.)

The otological part, which the author added for the convenience of students, is a condensed abstract of Roosa's work on the ear. We think, though, the author could have improved upon the original and enhanced the practical value of his abstract by adding a few lines on post-aural abscesses, and on the early signs of phlebitis of the lateral sinus, the diagnostic significance of which has recently been pointed out by Dr. J. O. Green. Also some information with reference to the relative merits of the ear-trumpets, audiphone, and kindred appliances would undoubtedly be highly appreciated by practitioners, for upon this topic they are very likely to be interrogated by friends and patients.

We are quite certain this book, by its plain, common-sense teaching, will make hosts of friends among medical students, and we hope it will convince them that they may become as competent to treat many diseased ears and eyes as to treat a sore throat, if they only pay as much attention to eye and ear dispensaries as they give to surgical and medical clinics. F. C. H.

ARTICLE XI.—THE SKIN IN HEALTH AND DISEASE. By L DUNCAN BULKLEY, M.D., Attending Physician for Skin and Venereal Diseases at the New York Hospital, Out-patient Department; Late Physician to the Skin Department, Demilt Dispensary, New York. Philadelphia: Presley Blakiston, 1880.

This excellent work is divided into four chapters. Chapter I is a *résumé* of the anatomy and physiology of the skin. Chapter II is devoted to the consideration of the "Care of the Skin in Health." This chapter is a most entertaining and instructive one. In the third section Dr. Bulkley endeavors to "give a gen-

eral idea of the way in which the skin becomes diseased," and "a sketch of the more prominent affections, and the means of their recognition and prevention as far as possible." The last chapter, "Diet and Hygiene in Diseases of the Skin," is filled with sound sense, brief and to the point.

This little book is one of the very best of the series of American Health Primers.

B. W. G.

ARTICLE XII.—CUTANEOUS AND VENEREAL MEMORANDA. By HENRY G. PIFFARD, A.M., M.D., Prof. Dermatology, University of the City of New York, Surgeon to Charity Hospital, etc.; and GEORGE HENRY FOX, A.M., M.D., Surgeon to the New York Dispensary; Lecturer on Diseases of the Skin, College of Physicians and Surgeons, New York, etc. Second Edition New York: Wm. Wood & Co., 1880.

Comparatively few medical men ever become familiar with the *nomenclature* of skin diseases. Only a small per centage of physicians are thoroughly informed upon this important branch of medicine. A considerable knowledge of the Greek and Latin languages, and especially the former, is *essential* to a *thorough* understanding of the vocabulary of diseases falling under the comprehensive term, DERMATOLOGY. No man, who is not fairly proficient in these languages, enjoys being brought face to face with such a word as "Trichophytasis," and to a reader whose knowledge of language is limited to the Anglo-Saxon, the idea of encountering such a word as "Telangiectasis," is anything but enjoyable.

A careful perusal of these "Memoranda" will serve to familiarize the student and practitioner with the names, as well as the symptoms and treatment of the principal diseases of the skin.

However, for a *complete* treatise upon this subject, one must consult other and more exhaustive works.

B. W. G.

BOOKS AND PAMPHLETS RECEIVED.

- A Treatise on Bright's Disease and Diabetes. By James Tyson, M.D.
- A Treatise on Albuminuria. By W. Howship Dickinson, M.D.
- A Treatise on the Materia Medica and Therapeutics of the Skin. By Henry G. Piffard, M.D.
- A Treatise on The Continued Fevers. By James C. Wilson, M.D.
- Cyclopædia of the Practice of Medicine. By Dr. H. von Ziemssen, Vol. IX.
- Medical Electricity: A Practical Treatise on the Applications of Electricity to Medicine and Surgery. By Roberts Bartholow, M.D.
- The Transactions of the American Medical College. Vol. XXXI, 1880.
- On the Antagonism Between Medicines and Between Remedies and Disease. By Robert Bartholow, M.D., LL.D.
- Dr. Paul Borners Reich's Medicinal Kalender für Deutschland auf das Jahr, 1881. Part II.
- Transactions of the Illinois State Medical Society, for 1878.
- Anatomical Plates. By Ambrose L. Ranney, M.D.
- How we Feed the Baby to Make Her Healthy and Happy, with Health Hints. By C. E. Page, M.D.
- Illinois State Medical Register for 1877-8. By D. W. Graham, M.D.
- The Diet Cure: An Essay on the Relations of Food and Drink to Health, Disease and Cure. By T. L. Nichol, M.D.
- An Introduction to Pathology and Morbid Anatomy. By T. H. Green, M.D.
- The Metric System in Medicine. By Oscar Oldberg, Phar. D.
- Lectures on the Diseases of the Nervous System, especially in Women. By S. Weir Mitchell, M.D.
- What Every Mother Should Know. By Edward Ellis, M.D.
- The Normal Temperature of the Head. By J. S. Lombard, M.D.
- The Diseases of Children; a Practical and Systematic work for Practitioners and Students. By William Henry Day, M.D.
- Medical Register and Directory, being a Treatise on the Diseases and Surgery of the Mouth, Jaws and Associate Parts. By James E. Garretson, M.D.
- Atlas of Human Anatomy, containing 180 large plates, arranged according to Drs. Oesterreicher and Erdl, from their original designs from nature and those of the greatest anatomists of modern times, with full explanatory texts. By J. A. Jeancon, M.D. In 12 numbers, from Parts 31-42.
- Supplement to Ziemssen's Cyclopædia of the Practice of Medicine. By George L. Peabody, M.D.

A Medical Formulary, based on the United States and British Pharmacopœias, together with numerous French, German and Unofficial Preparations. By Laurence Johnson, M.D.

The Principles of Myodynamics. By J. S. Wright, M.D.

Hygiene and Treatment of Catarrh. Therapeutic and Operative Measures for Chronic Catarrhal Inflammation of the Nose, Throat and Ears. Part II. By Thomas F. Rumbold, M.D.

The Hygiene and Treatment of Catarrh. Part I—Hygiene and Sanative Measures. Part II—Therapeutic Measures. By Thos. F. Rumbold, M.D.

Introduction to the Study of Indian Languages, with Words, Phrases and Sentences to be Collected. By J. W. Powell.

ALUMNI PRIZE.—The Prize of the Alumni Association of the College of Physicians and Surgeons of New York, of \$500, for an original essay on some subject connected with medicine or surgery, is open only to the competition of the Alumni of the College of Physicians and Surgeons.

The conditions upon which the prize will be awarded are as follows:

1. The subject is left to the option of the competitor.
2. The essay must present sufficient original, experimental or clinical observation to make it a useful contribution to medical knowledge.
3. The essay, designated by a motto, must be sent to a member of the Committee on Prize Essays, accompanied by a sealed envelope, inscribed with the same motto, and containing the name and address of the author, on or before April 1, 1882.

ALBERT H. BUCK, M.D., 109 Madison av.

CHARLES MCBURNEY, M.D., 40 West 36th street.

GEORGE L. PEABODY, M.D., 57 West 38th street.

Committee.

Editorial.

GENERAL EDUCATION AS PREPARATORY TO MEDICAL STUDIES.

The following brief paper was prepared for presentation to the meeting of the American Medical College Association, in Richmond, last May. But as a quorum of delegates was obtained only once, and then only long enough to hear the report of the Secretary and elect officers for the ensuing year, no other business was transacted, and, of course, no papers presented for consideration. The subject of preliminary education, however, is of sufficient importance to justify the insertion of what we had written in these pages, though in form addressed to the members of the College Association. It is as follows :

The necessity for a reasonable amount of mental development and discipline, and the advantages to be derived from a fair knowledge of the common branches of education, including the mathematics, physics, and natural sciences, before entering upon the study of medicine, are too obvious to require either argument or illustration.

Yet it is well known to all the members of this Association that a large per cent. of those who enter upon the study of medicine in this country have neither the mental discipline nor the knowledge just alluded to ; and only a small number of the colleges enforce any rule on the subject. Of all the defects in our system of educating men for the practice of medicine and surgery, none are greater or productive of more injury to the community and to the profession itself, than this want of preparatory knowledge and discipline on the part of those who enter it. It narrows the field of mental vision ; limits the materials for comparison and criticism ; precludes the possibility of apprehending

the philosophy or reason for many things, and consequently compels the acceptance of the opinions of teachers and writers without question; and embarrasses every step of intercourse either with professional brethren or with the community. These embarrassments and limitations to the progress and usefulness of individuals necessarily involve corresponding limitations to the usefulness and honor of the profession as a whole. Improvement here at the threshold of the profession, is naturally the first and most necessary step towards the accomplishment of needed improvements in all other respects.

Admitting the correctness of the foregoing statements, two questions of paramount importance demand the serious attention of this Association. First, What should constitute the minimum amount of education required, to qualify a person to enter upon the study of medicine? Second, When, and by whom, should the amount be ascertained and the standard enforced?

In considering the first of these questions it must be borne in mind that true education embraces both mental discipline and the acquisition of knowledge. The first imparts that capacity for thought, analysis, and deduction so necessary for successfully studying the various departments of medical science and art; and the second furnishes the implements or materials for efficient mental action. To acquire the amount of mental discipline here indicated, certainly requires direct scholastic training sufficient to acquire a good practical knowledge of the common English branches, with the addition of algebra, mental philosophy, physics, physical geography, and botany; while the knowledge embraced in these several branches of study may be regarded as an essential prerequisite to a proper study and understanding of the several branches of medical science. Taking the amount of education here indicated as the minimum standard preparatory to entering upon the study of medicine, I come to the second question, namely: When, and by whom should this standard be enforced?

All must agree that the time when the standard of preliminary education should be enforced is when the student proposes to enter upon his professional studies. First, because the mental discipline and knowledge resulting from such prior education are

necessary to qualify him to prosecute his medical studies with facility and success. And second, because, to permit him to spend three years of time and a considerable sum of money in pursuing professional studies and then reject him for lack of acquirements which he should have had before he began, would be so manifestly unjust as to need no comment. If the standard is to be enforced when the student proposes to enter upon his professional studies, without appealing to legislative bodies, there are only three parties on whom the duty of such enforcement can be devolved, namely: the preceptor with whom the student is supposed to commence his studies—boards of censors or committees appointed by medical societies—and the medical college in which he proposes to receive instruction.

It requires but little reflection or a limited range of practical observation to show, that the relations of preceptor and pupil in medicine, at the present time, are so far nominal or fictitious, and withal so variable in the character and attainments of the parties concerned, that any attempt to obtain the enforcement of a standard of preliminary education by them would be entirely nugatory. An attempt to accomplish something in this direction has been made by some of the State and local medical societies. But the simple fact that the action of such societies, is binding on no one except their own members, and the further fact that one-third, if not one-half of all the practitioners in the United States are not members of any medical society, would render these society organizations entirely incompetent to enforce any general standard of education.

So far as influencing their own members is concerned they may accomplish much, and all their efforts in this direction should be encouraged.

It becomes evident, from the foregoing considerations, that the medical colleges are the only organizations belonging to the profession on which the duty of exacting a proper standard of preliminary education can be devolved with a prospect of success. We have fully arrived at that period of progress when those entering the profession so generally resort to the medical schools for the chief part of their instruction, that whatever conditions are exacted for the admission to these institutions will effectually

reach the whole body of medical students. Hence, if a certain amount of education was agreed upon as the minimum that would be required for admission to registration and attendance in all our medical colleges it would effectually accomplish the object desired. It is only necessary that the faculty and trustees of each college should require all applicants for registration to furnish written evidence either in the form of diplomas or certificates from respectable colleges, academies or high schools that they have a good knowledge of all branches previously mentioned in this paper, or demonstrate their possession of the same by sustaining a satisfactory personal examination before a standing committee of the faculty, elected for that purpose. If this important measure could be adopted and carried into execution simultaneously by all the colleges, or even by all the members of this Association, it would neither alter their relative positions in relation to each other, nor so far diminish the aggregate number of students as to seriously embarrass any one of them; and yet it would cause a rapid improvement in the educational status of the great body of American medical students. This is clearly proved by the experience of the few colleges in which a fair standard of preliminary education has actually been demanded as a condition for admission for a sufficient number of years to test its practical working. In 1875, the faculty and trustees of the Chicago Medical College, which is also the medical department of the Northwestern University, adopted the standard of preliminary education already mentioned in this paper, except that botany was not included, and appointed a standing committee composed of three members of the faculty, to personally examine all applicants for admission to any of the classes in the college, who did not furnish the written evidences of qualification already specified. The same committee has been continued until the present, and has annually performed the duties assigned to it with fidelity. During the first two years a little more than twenty per cent. of all the applications for admission were either withdrawn rather than risk the results of an examination or were rejected by the committee. This ratio has steadily decreased, and the last year only ten per cent. of the whole number of applications were either withdrawn or rejected. This decrease

does not result from any abatement of the preliminary requirements but from the uniformly better qualifications of the applicants, as illustrated by the fact that of the forty-six who graduated in 1875 only ten per cent. were graduates of literary colleges, while of the forty-six who graduated at the last public commencement, March 27, 1881, nearly thirty per cent. were graduates of literary colleges of good reputation, and ninety per cent. of the remainder had received a good academic training. Precisely similar results have followed the institution of admission examinations in the medical school of Harvard University.

President Eliot, in his last annual report, page 33, says, in reference to the medical school as follows: "The examination for admission was first held in 1877; and its good effects were so soon manifested that the faculty were ready, after only three years experience, to add to the requisitions * * * In this university, until the reformation of the school in 1870-71, the medical students were noticeably inferior in bearing, manners and discipline to the students of other departments; they are now *indistinguishable* from other students. A corresponding change in the medical profession at large would be effected in twenty years, if all the important medical schools of the country should institute a reasonable examination for admission." These statements of the President are fully sustained by the facts given in the annual report of the Dean of the medical school for the same years.

The simple truth is, that just so soon as the medical schools of this country establish any given standard of education to be required for *admission* to such schools, all persons intending to enter upon the study of medicine will make the required preparation without the least hesitation. But so long as the doors of the great majority of our medical colleges are open to all who come without the least regard to their general educational attainments, just so long will the evils and embarrassments that now mar the reputation and usefulness of the profession remain without essential improvement.

As stated, however, at the commencement of this paper, the necessity for the possession of a fair degree of mental discipline, habits of study, and actual knowledge of all those branches

usually included in a good academic course of education, as a preparation for entering upon a field of study so extensive and intricate as that of medicine, is too apparent to justify further comment. And I confess to a feeling of disappointment, that, in the framing of the articles of confederation for this Association, one of its first provisions was not an *article* prescribing a matriculation examination of fair character to be instituted by each college as a condition of membership in the Association.

To remedy this great defect, I now propose as an amendment to Article II, of the articles of confederation a fourth section as follows :

“*Section 4.* Each college member shall require of all applicants for admission or registration as medical students, adequate written or documentary proof that said applicants have a thorough knowledge of the English branches including algebra, botany, physics, physical geography, and mental philosophy, or shall sustain a satisfactory examination on the same topics by a competent committee appointed for that purpose. N. S. D.

THE WARREN TRIENNIAL PRIZE.—The Warren Prize Committee offer a premium of \$400 for the best Dissertation worthy of a prize, upon the following subject:—

“CHRONIC BRIGHT’S DISEASE (parenchymatous and interstitial nephritis). The Nature and Mutual Relations of the Derangements in the Circulatory and Secretory Organs.”

Dissertations should be forwarded to the resident Physician, Massachusetts General Hospital, Boston, on or before February 1, 1883.

J. COLLINS WARREN,

Secretary Physicians & Surgeons, Mass. Gen’l Hospital.

Condensations from Late Editorials in the Medical Press.

NO PREMIUM ON FOLLY.—Not long ago an American physician lost his life from diphtheria contracted in the attempt to clear a tracheotomy tube by sucking it. His act of folly was, in the opinion of some, hardly condoned by its punishment; by others it was lauded as an act of heroism. A recent number of a contemporary relates with satisfaction, that the Albert medal has been conferred upon a young surgeon of England—a Mr. Grie—who applied his mouth to the tube to restore respiration in a similar case. The editor of our contemporary makes this the occasion for some ironical remarks about the want of appreciation of such deeds in “the land of the free and the home of the brave.”

It is a pity, not that there should be this lack of appreciation, but that there should be any exception to it. It is time, indeed, that expressions of admiration for reckless and unthinking acts of folly should stop, and that men who have any influence in molding public sentiment should strengthen the opinion that it is not praiseworthy, but on the contrary censurable, for one to expose a valuable life in the hope of saving one of less value, and tenfold reprehensible when the risk is so enormous and the benefits are so unlikely to be obtained or kept as in the cases we have just referred to. The man who does such things is not brave, he is rash; and, hard as it may sound, the American who lost his life and the Englishman who received the Albert medal were both actuated by idiotic although courageous impulsiveness.—*Medical Times*, April 23.

CONVALESCENT HOSPITAL.—A daily contemporary recently called attention to the case of a poor consumptive, who shot himself after failing to obtain admission to a hospital because of the incurability of his disease. The paper in question takes occasion to refer to the disposition on the part of hospitals to refuse admission to incurables, and casts unpleasant reflections upon the motives which actuate them in so doing. There is no denying the fact that the majority of our hospitals are adverse to admitting incurables, simply for the reason that thereby help would be denied to many other cases which are curable, and which might otherwise die.

What is needed is a convalescent hospital arranged on an economical plan, furnished with the ordinary conveniences of a home, and supplied with medical attendance. No costly accommodations are required, neither would it be necessary that the buildings be situated in the city, on expensive ground. Hospitals could be built on the cottage plan in suburban districts, and be made to a certain extent self-supporting by the light work that could be easily performed by the majority of the patients. It seems strange, in view of the great good to be accomplished by the establishment of such an institution on a sufficiently comprehensive plan, that some definite means have not been taken to that end.—*Medical Record*.

MEDICAL, AS COMPARED WITH LEGAL FEES.—Whose fault is it that doctors are, as a rule, so poorly paid? Chiefly, it is the fault of the doctors themselves. First, in that they charge so little; second, in that they so seldom bring suits for recovery; and third, in that they detract from the value of each other's service. The gentlemen of the law are not guilty of any of these misdemeanors, and the victim of even an exorbitant fee would as soon think of resisting, defenseless, a whole gang of assailants, as of disputing a bill for legal services.

Doctors, on the other hand, simple, foolish fellows, do not support each other at all when it comes to the matter of fees. They feel insulted when they hear that a fellow-practitioner has received

an unusual fee. They never got that much themselves, and they know their own equal, if not superior skill. A favorite expression of a little envy is to raise the cry on a fellow-practitioner of "high charges." The regulars get the "regular" fees, and the big fees go to the quacks.

Another difficulty in the matter of fees comes from the side of the law. The gentlemen of the law have the highest appreciation of the value of their own precious services, but the lowest of that of medical men. Honorable judges on the bench award to the meanest lawyers for the cheapest kind of service round sums from private or public funds, while skilled practitioners of our art are not even paid for their loss of time. The young fledgling attorney will receive more pay from the city funds for a few days' defense of a vile criminal, by order of the court, than a ward physician in a year for taking care of the honest poor.

When Prof. Charcot received \$9,000 for a consultation, implying a trip from Paris to Moscow, it was thought worthy of note all over the world. Well, we know of a fee of \$40,000 received by an attorney at law in this city, for services in a case unknown outside the city limits. Five thousand dollars were paid to the attorneys, in Covington, for the settlement of an estate of \$210,000, all in personal property, equal to cash, and but one-fourth that sum to the two physicians who swore to three years' continuous service. And yet a third physician, called once in consultation in the case, refused to sign a paper certifying to the correctness of the medical bill, unless the two physicians would declare that every service had been jointly made.

May the day never come—and it never will—when medicine will merit the opprobrium which now rests, in the matter of money, upon the law, but may it come soon when a fairer recompense for service, whose effect upon the mind and body no one but a physician knows, will be granted without a grudge.—*The Cincinnati Lancet and Clinic*, April 30.

THE ONTARIO MEDICAL ACT.—An American graduate, if we understand the Ontario Act aright, is to make "his education equivalent to that required of graduates in Canadian schools,

passing the same matriculation, and attend the same number of six months' winter courses of lectures." Hence the American graduate, at the end of his three years of college study, of four and a half to five months, plus the period and assiduousness of his study at home during the remainder of each of the three years, cannot enter upon the practice of medicine—whatever his attainments—in Canada, unless he attends a fourth year in some Canada school. Such a law as this would require some of the leading medical lights of the world to go to school again. Think of Marion Sims, Robert Battey, Thomas, Barker, Emmet, Goodell, Flint, junior and senior, Loomis, Sayre, Draper, Dalton, Stillé, Bartholow, Hammond, Hamilton, Mitchell, Wood, Gross, etc., sitting as students in the Canadian schools of medicine!

From the correspondence we have had with doctors of the United States, and which we have seen from Canadians, we are free to say we do not believe any *intentional* discourtesy has been done the regular graduates of the recognized medical colleges of the United States by the *regular* practitioners of the Dominion. Yet, while this may be so, in view of all the evidence we have in hand, we still think there are national professional courtesies between Canada, as well as Mexico, and the United States—courtesies of a character which we have tried to indicate—which should always be observed. If any well-recognized Canadian or Mexican practitioner of medicine comes to the United States to see a patient, whether he be called in consultation or to attend a patient, let the courtesy be extended of allowing that recognized practitioner to come. Or, if the summons of similar character be sent a regularly recognized practitioner of the United States, to go to either of the classes of patients in either of these different countries, let him be accorded the courtesies of the medical profession, at large, throughout the civilized world, without arrest, or even the threatening of arrest by the courts, the police, or any member of the profession.

If such indignities as have, on several occasions been offered "regular doctors" of the United States, are persisted in, it will become proper for the Congress of the United States to enact some self-protective law in the premises. We trust the occasion

for such a Congressional enactment will never occur.—*Virginia Medical Monthly*, May.

The same medical law is enforced in the Provinces of Quebec and New Brunswick. What is needed is a clause in the Medical Act which would allow any foreign graduate of medicine to register, after passing a satisfactory examination before a board appointed for the purpose by the legislature or the medical colleges of Canada.

THE SOCIAL STATUS OF THE MEDICAL PROFESSION.—It has always been a grievance with the profession in Europe, that its social position has been placed too low. Of the three so-called "learned" professions it ranks the lowest, and it never has pretended to equal the military or diplomatic service. The bar and the pulpit, the sword and the gown, have, in all periods of English history, overawed the medical man.

There is reason in this. The medical man is a manual laborer, a "chirurgion," literally a hand-worker; his proceedings are "maneuvers," works of the hand; his art is that of a skilled laborer whose success lies in his manual dexterity; and his work is none of the cleanest. Another reason is that he is a tradesman. His hand is extended for the shilling which he earns. In this immediate grasping for money, there is believed to be something deteriorating, and so there is.

Not long since, Dr. J. Milner Fothergill excited the intense ire of some of his associates by describing, in a series of letters to an American contemporary, the present low social position of the country doctor in England. In fact his statements were correct.

It is not better in Germany. The physician there is far from holding an equality with even the inferior nobility. It has been a matter of wondering comment that a few have married into the higher classes. Last year Dr. Wilin, an eminent physician of Breslau, married a Princess of Wurtemberg, greatly to the disgust of her relatives, no one of whom, probably, was his equal in education and mental vigor. The celebrated surgeon, Prof. Esmarch, is another of the few instances.

Such examples are extremely rare. Probably it is in France that the distinguished physician obtains the highest social awards. There is a directness about the French mind which seizes the relations of social life more quickly than any other.

In this democratic country of ours such invidious distinctions do not obtain. There are no discriminations against any honest avocation. The man is judged by his merits and the success which they bring him. He gains whatever social life has to offer without difficulty, if he makes it an object to do so. He is little the better for belonging to a learned profession, and certainly nothing the worse. This, we may take it, is the proper light for him to be viewed in.—*Medical and Surgical Reporter*, April 23.

SMALL-POX IN LONDON.—The fatal cases of small-pox in London, which had steadily increased from forty-three to seventy-seven in the five preceding weeks, further rose last week to eighty-four, and exceeded the corrected average number in the corresponding weeks of the last ten years by thirty; thirty-nine were recorded in the Metropolitan Asylum Hospitals at Fulham, Homerton, Stockwell, and Deptford, six in the Highgate Small-Pox Hospital, and thirty-four in private dwelling houses. Of the eighty-four persons who died from small-pox within registration London, thirty-two had resided in the South, twenty-four in the East, nineteen in the North, five in the West, and three in the Central groups of registration districts. The eighty-four fatal cases included twenty-three of children under five years of age, twenty-four of persons aged between five and twenty years, twenty-six of persons aged between twenty and forty years, and eleven of persons aged upwards of forty years. The number of small-pox patients in the Metropolitan Asylums Hospitals, which had increased from 820 to 940 in the three previous weeks, further rose to 963 on Saturday last. The new cases of small-pox admitted to these hospitals, which had been 224 and 244 in the two previous weeks, were 227 last week. The Highgate Small-pox Hospital contained ninety-five patients on Saturday

last, against seventy-seven and eighty-five at the end of each of the two preceding weeks; twenty-nine new cases were admitted to this hospital during the week.—*British Medical Journal*, April 30.

THE PLAIN TRUTH ABOUT VIVISECTION.—The appearance of a new bill bearing on its back the names of Sir Eardley Wilmot, Mr. Samuel Morley, and Mr. Firth, and having for its specific object the *total* abolition of vivisection, supplies us with a not unwelcome opportunity of placing our views explicitly before the profession and the public. In short, an occasion has arisen for speaking the plain truth about vivisection, and we shall not shrink from uttering it.

Vivisection is a scientific necessity; and, inasmuch as it is just as idle to try to stay the progress of science as it is to oppose the rising tide on the sea shore, the attempt to suppress this particular mode of investigation cannot be ultimately successful. Notwithstanding the assertions made by men with warm and feeling hearts, it is an incontrovertible fact that nearly all we *know* of the living organism in health and disease has been learned by vivisection, either intentionally performed, or, as the phrase goes, “accidentally” accomplished. Vivisection has been practiced in all ages and countries, more or less extensively. It is the analytical method applied to the study of organic life, just as the demolition of the earth’s crust by the geologist’s hammer is the analytical method applied to inorganic nature. The fact that the living animal feels is an untoward contingency, but it cannot qualify the major consideration. The dissection of the dead organism is tributary or complementary, and may well be made preparatory, to the dissection of the living body; but in the long run we must find ourselves face to face with the grim truth that it is the *living* body which science requires, and sternly demands, to explore, and which sooner or later she will explore in spite of all the hindrances which sentiment and humanity may throw in her way. If vivisection is totally abolished in this country practical physiologists will have to go elsewhere. The

great bulk of the information which has enabled physicians to treat diseases with new precision in recent years, and which has already led to the lengthening of many useful lives, is the outcome of vivisection. The aim to minimize the recourse to it was humane, the attempt to abolish it altogether is absurd.

The bill before us writes its own epitaph in the seventh clause : " This Act shall not apply to inter-vertebrate animals." If no pain is to be inflicted in the interests of science we must desist from vivisection of every kind. It is a contemptible affectation of pharasaic humanity to take credit for protecting the dog while we allow the frog to be dismembered for our information and advantage. If a more stringent act on this subject were passed to-morrow, the average number of vivisections now performed would not be reduced. The legislative exploit would simply be offering a little additional incense to our national vanity. The operations denounced here would be performed elsewhere; and then, as now, we should hug the comforting sense of superior humanity, while we bid eagerly for the increased information science might gain in her "cruel" quest.—*London Lancet*, May.

THE "HAMMOND PRIZE" OF THE AMERICAN NEUROLOGICAL ASSOCIATION.—The American Neurological Association offers a prize of five hundred dollars, to be known as the "William A. Hammond Prize," and to be awarded at the meeting in June, 1882, to the author of the best essay on the *Functions of the Thalamus in Man*.

The conditions under which this prize is to be awarded are as follows :

1. The prize is open to competitors of all nationalities.
2. The essays are to be based upon original observations and experiments on man and the lower animals.
3. The competing essays must be written in the English, French or German language; if in the last, the manuscript is to be in the Italian handwriting.
4. Essays are to be sent (postage prepaid) to the Secretary of

the Prize Committee, Dr. E. C. Seguin, 41 West 20th street, New York City, on or before February 1, 1882; each essay to be marked by a distinctive device or motto, and accompanied by a sealed envelope bearing the same device or motto, and containing the author's visiting card.

5. The successful essay will be the property of the Association, which will assume the care of its publication.

6. Any intimation tending to reveal the authorship of any of the essays submitted, whether directly or indirectly conveyed to the Committee or to any member thereof, shall exclude the essay from competition.

7. The award of the prize will be announced by the undersigned Committee; and will be publicly declared by the President of the Association at the meeting in June, 1882.

8. The amount of the prize will be given to the successful competitor in gold coin of the United States, or, if he prefers it, in the shape of a gold medal bearing a suitable device and inscription.

Signed:

F. T. MILES, M.D., Baltimore.

J. S. JEWELL, M.D., Chicago.

E. C. SEGUIN, M.D., New York.

By the opening of the fourth course of lectures in October next, the Washington Training School for Nurses hopes to be able to rent a building and have a comfortable house for the nurses, where their training may be systematically conducted under an experienced and educated head nurse, and where the lectures may be given by the medical faculty until a general hospital is established, in which the training of nurses will, we trust, be made a legitimate part of its functions. As an encouragement to those who contemplate entering upon the calling of the "trained nurse," they can state that all of their advanced pupils have found in Washington full and remunerative employment even before they had finished their studies, and only consented to assume the responsibility under the most urgent appeal, but they have in every case given satisfaction alike to the sick and the physician in attendance.—*Dr. Toner.*

SOCIETY MEETINGS.

Chicago Medical Society—Mondays, July 4-18.

West Chicago Medical Society—Mondays, July 11-25.

Biological Society—Wednesday, July 6.

CLINICS.

MONDAY.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Prof. Holmes; 3 p. m., Otological, by Prof. Jones.

Mercy Hospital—2 p. m., Surgical, by Prof. Andrews.

Woman's Medical College—2 p. m., Dermatological and Venereal, by Prof. Maynard; 3 p. m., Diseases of the Chest, Prof. Ingals.

TUESDAY.

Rush Medical College—3 p. m., Dermatological and Venereal, by Prof. Hyde.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Quine.

WEDNESDAY.

Chicago Medical College—2 p. m., Eye and Ear, by Prof. Jones.

Rush Medical College—2 p. m., Medical, by Dr. Bridge; 3 p. m., Ophthalmological and Otological, by Prof. Holmes; 3:30 to 4:30 p. m., Diseases of the Chest, by Dr. E. Fletcher Ingals.

THURSDAY.

Chicago Medical College—2 p. m., Gynæcological, by Prof. Jenks.

Rush Medical College—2 p. m., Diseases of Children, by Dr. Knox; 3 p. m., Diseases of the Nervous System, by Prof. Lyman.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Dr. Hotz.

Woman's Medical College—3 p. m., Surgical, by Prof. Owens.

FRIDAY.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Davis.

SATURDAY.

Rush Medical College—2 p. m., Surgical, by Prof. Gunn; 3 p. m., Orthopædic, by Prof. Owens.

Chicago Medical College—2 p. m., Surgical, by Prof. Isham; 3 p. m., Neurological, by Prof. Jewell.

Woman's Medical College—11 a. m., Ophthalmological, by Prof. Montgomery; 2 p. m., Gynæcological, by Prof. Fitch.

Daily Clinics, from 2 to 4 p. m., at the Central Free Dispensary, and at the South Side Dispensary.

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Original Communications.

ARTICLE I.

ECLAMPSIA. PUERPERAL CONVULSIONS. By H. Z. GILL, A.M., M.D., Physician to the Southern Illinois Penitentiary at Chester, Ill. Read before the Jersey County Medical and Surgical Society.

The comparative frequency of puerperal convulsions is not agreed upon by obstetricians of different countries, but varies from one in two hundred to one in four hundred cases of labor, when taking large numbers of patients.

The convulsions may appear at any period of gestation, from the first month of pregnancy to the last, and even during the month following delivery. The last two months of pregnancy form the most susceptible period for the appearance of the disease. According to most statistics the following is given as the order of frequency as to the period of first attack: During labor, following delivery, and during pregnancy. But M. Bailly,* from whom I shall draw liberally, affirms that this is not the exact state of the

* Nouveau Dict. de Méd., et de Chirurgie Pratiques. Tome 12, Art. Eclampsie.

facts. He says, generally it is during the course of the ninth month that the convulsions set in, and that labor is the consequence of the attack, hence he changes the order to the following: Pregnancy, labor, following labor.

Symptoms—Prodromic.—Occasionally the attack may come on without any warning being observed by patient or attendants. This has even been denied, and has been charged to inobservance. There are three prominent symptoms giving warning of an attack of puerperal eclampsia (as well as frequently others of less significance), namely—pain in the head, disturbed vision, and epigastric pain or oppression. Other symptoms, less common and less pronounced, are—vomiting and dyspnœa, disturbance of intellection and of sensation, vertigo, tinnitus aurium, drowsiness or sleeplessness, and excitability.

Symptoms of the Paroxysm.—The paroxysms are remarkably similar in their general characteristics. The difference is mainly in degree of severity and duration. When once seen, the picture will be sufficiently familiar, ever after, in all its principal points. A brief description may be admissible. After having experienced for a short time the symptoms above described, the patient seems more absorbed and sunk in profound meditation, then her gaze becomes fixed for a few moments, and the paroxysm commences immediately by rapid contractions of the muscles of the face, the eye-lids and the eye-balls, which latter are seen to roll about in their sockets. These jerking movements, which give to the countenance a distorted expression, most painful to witness, quickly give place to tonic contractions of the same part and of the neck. The mouth at first is strongly drawn to the left, and the face is slowly drawn towards the shoulder of the same side. The eye-balls, turned upwards by their levator muscles, leave exposed but a small inferior segment of the sclerotic between the partially opened lids. After being slowly turned toward the left, the face, by a slow movement in the opposite direction, is turned toward the right shoulder. The convulsive agitation is rapidly extended from the head to the body. The extensor muscles of the trunk, powerfully contracting, tend to curve the spine backward (opisthotonos.) The entire body becomes ex-

tremely rigid. The extremities are equally rigid and are generally extended. The hands, forcibly pronated, are closed, the thumb in the palm of the hand, and the fingers closed tightly over it. More rarely the predominant action of the flexor muscles has the effect of immobilizing in a semi-flexed position the different parts of the upper extremities.

The diaphragm itself and the muscles of respiration finally become implicated. The respiration becomes suspended, and the face congested, flushed and purple. The tongue, projected from the mouth at the onset of the paroxysm, is seized and lacerated by the spasmodic gnashing of the jaws, and the blood which escapes from the wounds thus made gives to the mucus which is discharged upon the cheeks, a bloody tint. The muscles of the larynx, and perhaps those of the throat, strongly convulsed, close more or less completely the orifice of these cavities. The result is that the air, compressed by the convulsive contractions of the chest, escapes only with difficulty, producing a peculiar wheezing often quite marked. At the same time there is a complete suspension of intellection and sensation. The patient neither sees, hears, nor feels. An external irritant does not seem to be recognized.

Immediately after these tonic convulsions, those of a clonic character follow, which, like the former, extend to the entire muscular system. The general rigidity of the preceding period is followed by jerking movements, which occasion rapid agitation of the head, trunk and extremities. The result is, the most horrible grimaces are produced by the irregular movements of the mouth, the eyelids and even the eye-balls. Inspiration, until this time completely suspended, begins to be re-established. An interrupted and stertorous respiration produced with noise, expels bronchial and bloody sputa from the mouth. The agitation of the trunk and extremities consists of limited jerks which shake the body without displacing it; so that it is never necessary to hold the patient by force to prevent her from injuring herself as is the case in hysterics.

The cutaneous and visceral congestion caused by the interruption and disturbance of the respiration, increases during the first

period. It reveals itself at the periphery of the body by the injection of the ocular conjunctiva and the subconjunctival ecchymosis, by cutaneous discoloration, by the heat of skin and finally by an abundant perspiration which inundates the entire body. The pulse, full and strong at the beginning of the convulsions, becomes rapidly accelerated under the influence of the muscular and respiratory disturbance, and finally acquires an extreme frequency, at the same time that it becomes so weak as to be almost imperceptible, when the convulsion has attained its last stage. All these symptoms become ameliorated toward the end of the paroxysm. The respiration and circulation, at first interrupted or disturbed, becomes regular; the congestions, both superficial and deep, disappear; the cutaneous discoloration becomes natural again; the jerking of the extremities and trunk grows more feeble and farther apart, and at last cease entirely to give place to a more or less complete restoration.

Résumé—Two well defined stages or periods exist in the convulsive manifestations of eclampsia. The first, the tonic convulsion, lasts about twenty seconds. The second, the clonic, which succeeds the former, is much longer and generally continues from one to five minutes, and may even exceed the latter duration. The gradual re-establishing of the respiration during this period explains the fact that it may continue so long without producing death. The period of tetanic convulsions alone approximately threaten the life, and the cases in which the patients have died during the paroxysm are very likely owing to the uninterrupted duration of the tonic convulsions.

Symptoms—During the Interval.—Coma, more or less profound, and more or less prolonged, follows the paroxysm, with loss of intellection and sensation.

The approximate cause of the coma is not doubtful. It depends upon a violent cerebral congestion, and sometimes upon a serous effusion into the brain, the existence of which is sufficiently clear by the comatose symptoms during life as well by the anatomical proof on autopsy. This congestion is consecutive to the convulsion, and is the result of the obstruction to the return of blood from the brain, both by the contraction of the muscles of the

neck, and the suspension of respiration during the paroxysm. It differs from the coma of Bright's disease, in being secondary to the paroxysm, and not an initial symptom as in this renal disease.

Chemistry has shown two changes in the blood: A diminution of urea, and a diminution in the per cent. of albumen from 70 to 60, and even still lower, in the 1000. The changes in the urine are not less important than those in the blood. It contains a greater or less quantity of albumen. The attention of the physician being called to the existence of some of the premonitory symptoms, anasarca or some of the nervous symptoms, the urine is examined for a few days in succession, and is found to contain albumen, varying somewhat from day to day in quantity. The amount may be ascertained approximately by trial in the ordinary test tube, and observing the proportion that deposits at the bottom on standing. By such examination previous to, during, and after the attack, we ascertain the following facts: 1. The proportion of albumen varies during the premonitory symptoms, from day to day and may even disappear. 2. That its quality increases greatly during the attack under the influence of the renal congestion which accompanies it. 3. That it diminishes quite rapidly after delivery, when the disease is to have a favorable termination. 4. That the continuance of the albumen, in any considerable quantity beyond two or three weeks after the attack, denotes a more serious change in the kidneys, and adds greatly to the gravity of the prognosis.—*Bailly*.

According to Braun, the urea is always diminished in its proportion to the 1000 parts, and sometimes it is altogether absent. The uric acid is also generally in small quantity. The other constituents of the urine vary considerably. The uroxanthine is in increased quantity. The quantity of urine is always much diminished, and in some cases may even extend to suppression, and the suppression, partial or complete, may continue for several days. It is generally of a light amber color, and usually remains clear on cooling, but not always; an abundant precipitate of the earthy salts may form and give it a milky appearance, which disappears on heating. The light-colored hazy specimens of urine usually contain the largest amount of albumen.

The specific gravity varies from 1010 to 1030. In the deposit formed in the urine, in the first 24 hours after evacuation, besides the mucous and blood globules and epithelial cells of the ureters, some fibrinous casts of the tubes of the kidneys may be found. If, however, the urine is alkaline they will be dissolved by the bicarbonate of ammonia developed by decomposition. They proceed from the uriniferous tubules and are the result of coagulation of the exuded albumino-fibrous exudate, and mark the transition from the first to the second degree of albuminous nephritis.

The *duration* of the disease is *brief*, the *course* is rapid, the termination is one of three: Recovery, death, or the development of some other disease resulting from the convulsions. The most frequent result is recovery, either quite prompt and complete, or more or less protracted, dependent upon the degree of renal lesion, and the cerebral injury. In the more fortunate cases the casts disappear from the urine in two or three days, the albumen in from six to ten days after delivery, and the œdema is removed in about a week.

Death is a very frequent termination of this terrible disease. Great diminution of the urine with an amount of albumen sufficient to cause coagulation of almost the entire amount of urine, under the action of nitric acid, violent cerebral symptoms following paroxysms at short intervals, form a totality of symptoms sufficient to strongly indicate a fatal termination.

Death may occur during the tonic spasm when that continues more than half a minute. But this is exceptional. It usually occurs during the stage of coma. It seems then to result from the nervous disturbance, and from the derangement of the oxygenation of the blood produced by the repetition of the paroxysms. Nothing can rescue the patient from the profound coma into which she is plunged, the pulse is increased in rapidity, the respiration becomes more and more embarrassed, and a low progressive asphyxia puts an end to life.

It is both asserted and denied that violent and irregular contractions of the uterus, caused by the convulsions, may occur.

Hæmorrhage into the brain may occur, beyond question. It, and the effusion into the brain are the result of the afflux of blood

to the brain during the convulsion, and is characterized by paralysis, a symptom which is absent in uræmic cerebral disease, not complicated with cerebral hæmorrhage. Of course apoplexy cannot be recognized until the general muscular relaxation has subsided in order to ascertain the inequality of the action of the muscles of the two sides. It has occurred that the general relaxation of the muscles and the deep coma have so masked the paralytic condition that the cerebral hæmorrhage was only discovered at the autopsy. This same sanguineous effusion may give rise to one of the complications of eclampsia, to-wit, meningitis, from which the patient may at a later date succumb.

Other accidents may follow eclampsia, viz.: Congestion and apoplexy of the lung; violent concussion of the abdominal viscera, and the painful manipulations may produce inflammations from which the patient may die after having escaped the immediate dangers of the convulsions.

It is now pretty generally believed that an albuminuric patient is more likely to suffer from uterine hæmorrhage.—*Blot*.

According to the researches of Imbert-Gourbeyre, chronic Bright's disease follows in about one case in ten.

Only in a treatise on the subject of eclampsia would it seem necessary to speak of the diagnosis. The cases are usually so well marked as scarcely to admit of error on this point.

The mortality varies from one in two to one in three, according to different authors. In a large number (119) collected by Bailly, the deaths were 51 in the 119 cases—42.85 per cent. The proportion of deaths will depend upon the degree of one or more of the following symptoms: The degree of albuminuria, the violence of the nervous attacks and the rapidity of their succession, and the depth of the coma and insensibility. The period at which the attack comes on will have an influence on the prognosis. Aside from the paralysis or paresis which follows the cerebral effusions, the cases generally recover from the other functional effects of the disease. The disturbance of vision and partial amaurosis quite frequently continue for some weeks, but in the end generally disappear. While labor is precipitated by the convulsions, their repetition is also excited by the pains of labor.

Upon the general acceptance of the above view the profession is in accord as to the advisability of a speedy delivery at as early a date as the maternal condition will admit. The following facts from Braun will make this point clear: After the delivery of the child and the removal of the after-birth, the attacks suddenly ceased in 37 cases out of a hundred, they became weaker in 31 cases, and they continued with the same severity in only 32 cases in the hundred.

Eclampsia, while a very dangerous disease for the mother, is still more fatal to the child. In the majority of the cases the death of the foetus is anterior to its birth, and results mainly from the disturbance of the uterine circulation arising from the nervous paroxysm. The foetus may be dead for a number of days before the attack, and the death may be the result of the deteriorated condition of the mother's blood, or the manipulations in delivery may cause the death of the child; or again, it may die within a few days after delivery from debility.

Pathological Anatomy. — Investigations of the structural changes of the nerve centers, as connected with eclamptic convulsions, have been thus far rather negative in their results. These anatomo-pathological changes are either so transient or so slight as to evade the investigator. In many cases, dead of eclampsia, no trace of structural change can be found in the brain or in the envelopes. In others there are modifications recognizable, but they are scarcely of such a nature and degree as to account for the intensity of the effect—the convulsion. In fact, aside from the effects common to other parts of the body, viz., serous effusion, the more marked modifications are the result of the convulsions, and follow them rather than being the cause of them. I have previously spoken of the effusion, and occasionally of a hæmorrhagic lesion within the cranium. This frequent absence of any organic lesion of sufficient significance to account for the results, has caused the investigation to be extended to the fluids of the body, and to attribute the functional disturbances which form the symptomatology to changes in the blood. Theory and observation seem both to sanction such a view.

“The only constant organic changes in the eclamptic woman are those of which the kidneys are the seat. If they have es-

caped a certain number of observers it must be attributed to the want of proper means used by them in the investigation. Indeed it is alleged, since the works of Frerichs (1851), Bach and Imbert-Gourbeyre (1856), that in the kidneys of all subjects dying from paroxysms of eclampsia, the anatomical alterations which characterize the different degrees of Bright's disease, are observed. The existence of these lesions cannot be the subject of any doubt. The naked eye is not ordinarily sufficient to ascertain them, but the microscope reveals them where the unaided vision is unable to discover them. * * The alterations of the kidneys, I repeat, are never absent to those who know how to seek them with proper means, and constitute, properly speaking, with the physico-chemical alterations of the blood and urine, the pathological anatomy of eclampsia."—*Bailly*.

The only general condition which seems to have a positive value in predisposing to eclampsia is the puerperal state. The primiparous are far more liable to the disease than the multiparous, since four out of five cases of eclampsia are of the former class. In my own observations they have all been of the former class, though that was a mere coincidence. The principal predisposing cause is connected with the structural and functional condition of the kidney. Without arguing the case, or giving the authorities in favor of the view that albumen is to be found in all cases of eclampsia, though it may not be found at every examination, I assume that it may be found at various times; and the opposite condition is of so rare occurrence as to have no effect upon the fact of its existence in all cases when carefully sought for at periods sufficiently frequent. Blot, in examining 205 women, who came into the lying-in ward of the Maternity Hospital, found albuminuria, more or less pronounced, in 41—that is, one in five—all supposed to be under the influence of labor. The proportionate number albuminuric, taking the states of pregnancy, labor and lying-in, is not definitely known from exact research. The close relationship which exists between eclampsia and puerperal albuminuria, whether as to cause and effect, or of community of origin, adds to the importance of the study of the origin of either. for it may be that the discovery of the cause of one will be to make known the cause of the other.

The questions, as Bailly puts them are: Is the albuminuria of the puerperal state the result of an affection foreign to the kidneys, or is it connected with a pathological condition of these organs? And in the latter supposition, does it depend upon a simple functional disturbance of the gland; or, on the contrary, is it linked to a constant and primitive lesion of this same gland—albuminous nephritis, acute Bright's disease; or again, is it due, as the case may be, to the one or the other of these mechanisms, or successively to both, in the same woman. These different views have been supported, and are to-day held, by men of great attainment. Again, another view is that the urinary disturbance being at the beginning only functional, in time produces an organic alteration, which latter in its turn produces albuminuria. It has been still further argued that at one time it may depend upon a functional disturbance, at another an organic. I am disposed to believe in a definite condition, especially as to the cause of albuminuria (leucomuria) of the later months of pregnancy.

1. The theory of the supra-albuminous condition of the blood (of the solid portions not of the serum), while being interesting in some of its features, is far from being able to explain many of the accompanying facts.

2. Puerperal albuminuria is caused by the serous polyæmia, which the puerperality produces. This position was held by some of our prominent writers on obstetrics, among whom we find Maugenest, an author of repute (1867). This is rather supposed than proven in its assumed facts.

3. Puerperal albuminuria is produced by a temporary, or by a permanent disease of the kidneys. To this proposition the majority of authors hold; and the number is daily increasing. And the works of Bach, Bailly, Litzmann, Frerichs and Braun no longer permit of doubt that the albumen in the urine of the pregnant woman has its origin in the pathological condition of the kidney. Microscopic examinations of the kidneys of subjects dead of eclampsia, show beyond question the nature of the morbid change, viz., acute Bright's disease or albuminous nephritis. Imbert-Gourbeyre was the first to insist upon this constant relation or the co-existence of acute Bright's disease

with eclampsia. Bailly is especially emphatic on this point, as expressed in the following sentence: "Therefore, for us at least, without doubt * * we admit that albuminuria, connected with eclampsia, finds its cause exclusively in an acute Bright's disease."*

The *modus operandi* by which the compression and restraint upon the renal circulation is produced has been so clearly set forth by Braun, as to justify our quoting it here: "The first stage of acute Bright's disease consists in a hyperæmia caused by the congestion of the venous blood, affecting one or both of the kidneys at once. Upon this state follows, immediately, an extravasation of fibrinous exudation into the malpighian corpuscles, which invades to a certain distance the interstitial tissue, envelops the vascular net-work, escapes partly into the urinary canaliculi and often to be secreted in a fluid form with the urine. However, most commonly the albumen of the exudation passes alone with the urine, while nearly all the fibrinous matter coagulates in the tubuli and in the cortical substance. The latter remains there for a longer or shorter time, till it is expelled at the same time that the exfoliated epithelium is evacuated with the urine in the form of the tubes of Bellini and of Ferrine. The nutrition of the uriniferous tubes at last suffer from the inflammatory condition and from the production of an abnormal plasma around their epithelial cells; the tubuli are transformed by a retrograde metamorphosis into cells filled with a detritus rich in fat; a similar change takes place at the same time in the fibrinous matter, which has remained there long. The walls of the tubuli, deprived of their epithelium, actually destroyed, come in contact, and the sojourn of the false membrane gives rise to a fibrous cicatricial tissue, amorphous, which displaces the glandular parenchyma and causes the depressions which we observe on the surface of the kidneys; we observe at the same time, upon the external surface or in a section of the cortical substance, other tubuli still filled with fat. The more the destruction of the tubuli has advanced the more of course is the volume of the

* Loc. cit., p. 320.

kidneys reduced, since at the same time a part of the circulatory apparatus of nutrition is also destroyed by obliteration."

While this may very clearly explain the cause and condition of things in the later months of pregnancy, it does not seem an adequate explanation for the leucomuria of early pregnancy; and Braun also says that future observations must decide whether eclampsia of the early months of pregnancy is or is not co-existent with Bright's disease. The two do not necessarily stand to each other as cause and effect; but they are both rather as symptoms of a common cause, and not co-existing by mere accident. The proximate or determining cause of eclampsia is very difficult to determine positively. Symptoms which are so uniform in character should have some definite cause or constant condition to produce them. And it is this definite cause which we should strive to discover. Various theories have been put forth, some of which refer the determining cause to material changes in the brain or spine; some to reflex irritation; some to anæmia of the brain, and lastly others to the composition of the blood as not being appropriate for the healthy nutrition of the nervous system (brain and spine). The latter view has the greatest number of adherents. If this be the true theory, so far as it goes, then what is this special deviation from the normal condition? Uræmia had its day and was succeeded by ammonæmia—Frerichs holding that the change of urea to carbonate of ammonia took place in the blood, and Treitz that the change took place in the intestine and was absorbed into the blood, hence the term ammonæmia as applied by Jaccoud. Still another theory of the pathogeny of eclampsia is, that it depends upon other retained materials of denutrition, viz., *extractive matters* of the blood. It is claimed by Bailly that this explanation has the greatest number of scientific supporters, because it rests upon an undeniable fact. The exact nature of the blood poison is not yet fully demonstrated, at least not to the satisfaction of chemists, and we must await further developments at this particular point.

TREATMENT.

The treatment of eclampsia naturally divides itself into preventive and curative. The preventive treatment, made use of

during pregnancy, will be chosen and applied according to the existing symptoms. Blood-letting will seldom be necessary; it may be employed in cases giving evidence of local congestions and disturbance of the brain when found in plethoric, full-blooded individuals. My own practice has been to keep the emunctories active, especially giving attention to the action of the bowels by the administration of aperients or cathartics, as may be indicated; and to the kidneys by the use of alkaline diuretics, in combination with digitalis.

During labor it is important to diminish its *severity* by various anodyne medicines; and to abbreviate its *duration* as much as practicable, in order to lessen as far as possible the exciting causes of the convulsion. If the individual have an abundance of blood, venesection, at a time when the symptoms are such as to indicate the imminence of an attack of convulsions, would be proper. More frequently, however, the quieting remedies, of which morphine and chloral stand preëminent, would be preferable. Morphine administered hypodermically has been in use in this disease for many years. I saw it used more than twelve years ago in the wards of Prof. Braun in the Vienna Hospital, both when the attack was threatening and after it had occurred. Chloral, in doses sufficient to render the patient in decided measure insensible to pain or excitement, is a remedy until quite recently much relied on, and is at present held in high favor by some.

It may be eminently proper to terminate the labor by artificial means under the same general conditions as would be advisable after one or more convulsions has occurred, of which I shall speak later.

The curative treatment is *medical* or *surgical*.

After a convulsion has occurred, or even when one has announced its near approach by the peculiar symptoms immediately preceding it, there are two principal therapeutic measures to be considered, viz., venesection and narcotics; and one or both of them should be brought into requisition at once. If the patient is moderately full blooded, bleed moderately; if plethoric, bleed freely. Follow this immediately with a full dose of morphine hypodermically, or with chloral in forty or sixty-grain dose, given *per os* or *per anum*, as circumstances may indicate.

The question of blood-letting has been much discussed, many favoring it—regarding it above any other measure. It has received the sanction of a greater number of the profession than any other remedy. At the same time we may not fail to record the fact that many able men in the profession prefer to depend upon other means. This is not the time to enter into the full discussion of the reasons assigned on the one hand in favor of, and on the other against, the practice. The amount of cerebral congestion found *post mortem* in many cases, would seem to indicate not merely the propriety but the positive demand for this practice, ordinarily so promptly relieving intercranial congestion. From ten to twenty ounces taken in a bold stream will give efficiency to the other remedies. This measure is as old as the history of the disease.

The next remedy in order is *morphine*; and to get the effect as rapidly and as decidedly as possible it should be given hypodermically. This remedy, and this method of using it, is highly endorsed and has been followed by remarkably favorable results, especially so when used in heroic doses. It is gaining in favor with the profession. In the July (1880), number of the *American Journal of Obstetrics*, Dr. C. C. P. Clark gave a very enthusiastic report of the use of large (two-grain) doses of morphine hypodermically administered. In choice and vigorous language he speaks quite disparagingly of our pathological knowledge of the disease and of the general treatment also, except of the heroic doses of morphine. But we cannot yet adopt his over sanguine expression, "When obstetricians shall fearlessly follow out the rules that I laid down, this fell disease will cease to be a terror."* He cites several cases successfully treated by his heroic plan; and Dr. C. P. Faust, of Graham's, S. C., follows in the same train.

Chloral finds very zealous advocates in recent years among distinguished members of the profession. In a recent article by Dr. H. H. Kane (*American Journal of Obstetrics*, April, 1881), on "Chloral Hydrate, its Uses in Obstetrical Practice," the following language may be found: "It has, however, cured more

* *American Journal of Obstetrics*, April, 1881, p. 416.

cases than any single remedy or combination of remedies known to us." If we should take the assertions of Drs. Clark and Kane, above quoted, we might conclude that puerperal eclampsia had become, under these forms of treatment, shorn of its terrors and of most of its former fatality. We do not, however, share in such enthusiasm. Medical statistics, to be of value, must include large numbers, and the influencing circumstances must be held strictly to account. Dr. Wm. Goodell, as reported in the Proceedings of the Philadelphia Obstetrical Society (*American Journal of Obstetrics*, April, 1880), says he has never lost a case; and that he was one of the first in the city to use chloral in eclampsia. He also spoke highly of the hypodermic injection of morphia after venesection. The best effects seem to be obtained when administered as an enema sufficiently diluted with syrup or beaten egg, and introduced well up into the intestine: the initial dose should be from forty to sixty grains, according to the urgency of the case. *Chloroform* is a kindred remedy of much repute also. Some years ago Braun reported sixteen cases thus treated, all resulting in recovery. Very few, if any others, have been equally successful, and many able authors have rejected it. As in any other case, it should be given with caution, but may be continued for many hours. Our own preference would be to use it in conjunction with other remedies.

There are other remedies, of less efficiency, which need only be mentioned here. In case of the urine being albuminous, act upon the kidneys with infusion of digitalis and acetate of potash. A free cathartic may be given later—compound jalap powder or ol. tiglin.

The surgical management is of great importance to abbreviate the duration of labor, and to mitigate or terminate the irritation and suffering. Delivery should be effected as soon as practicable. If the os is dilated or dilatable, Paul Dubois says, "evacuate the uterus just as soon as it can be done without violence." The wisdom of this advice applies to both mother and child. Braun reports the fact that of one hundred cases of eclampsia the convulsions ceased promptly after the expulsion of the fœtus and after-birth, in fifty-seven (57) cases; grew weaker in thirty-one

(31), and continued with the same violence in only thirty-two (32). If labor has not commenced and the convulsions do not yield to the usual remedies—morphine, chloral and chloroform, I should advise delivery by artificial aid, especially so if the patient is albuminuric. In this case the os must be dilated by means of the hand or other approved means, and delivery effected by the best means which the circumstances require.

The three following cases will show the treatment, which in my judgment is suited to the disease, or threatened disease, in the three periods—*before, during and after* labor :

I. Mrs. H., aged thirty, primipara. Considerably oedematous in both upper and lower extremities. Urine albuminous to the extent of one-third. No casts found. Suffered at times considerably with headache. These conditions existed for some weeks before confinement. Gave pretty freely of comp. jalap powder ; also infusion of digitalis and acetate of potash. Labor came on at the expected time ; and was moderately easy. Patient was considerably nervous ; but there was no serious complication, and the recovery was rapid and complete.

II. Mrs. S., aged twenty-six, primipara. Labor at full term. (There had been no symptoms such as to attract attention previous to labor, which was somewhat tedious). Complained at one time of headache. First convulsion came on before the os was dilated or dilatable. In half an hour a second paroxysm came on. The os being now dilatable, delivery was promptly effected with the forceps. The child was still-born, but was resuscitated after considerable effort. Morphine was given after the first convulsion, by the mouth. Two convulsions occurred after delivery. Bled after the third to about eight ounces. Gave infusion of digitalis and acetate of potash. Urine after delivery was loaded with albumen and casts. Consciousness did not return for about thirty hours. The diuretic acted well. Mother and child recovered. The former had an attack of neuralgic rheumatism four months previously, threatening abortion. Has been delivered safely twice since, the last time followed by a severe attack of inflammation in the uterine region, and has had attacks of rheumatism since getting up. On November 16

(date of last confinement), when I first saw her, the os was fully dilated; membranes intact; pains short, not expulsive to any considerable degree; much pain in the right inguinal region, and frequently extending down the limb. A short time before the delivery of the head there was mental aberration for the period of half a minute or more. She said her head ached, and she asked, "Go where?" But it soon passed off. The delivery was effected without special difficulty. On the previous day the urine showed a decided trace of albumen, and a specific gravity of 1010.

III. Mrs. W., aged 43. primipara. Saw her at one o'clock, A.M., in consultation. Head of child resting on perineum. Body of patient œdematous, lower extremities very much so. An hour or more previously she had complained of a severe pain in her head and dimness of vision. About twenty minutes previous to the time of my seeing her she had a convulsion, and was now entirely insensible. She had taken some chloral. Proceeded to deliver with forceps; but during the preparation she had a second severe convulsion, immediately after which delivery was effected. Gave chloral by the mouth, and morphine hypodermically. One (or two?) convulsions occurred after delivery. The coma being very profound I bled her eight or ten ounces, there having been a moderate loss otherwise. No more convulsions. Examined the urine drawn after delivery, and found it three-fourths albumen, and a later examination showed an abundance of casts, under the microscope. No examination had been made, strange to say, for about two weeks. Gave digitalis infusion and acetate of potash. Consciousness began to return in about twelve hours, then gave a free cathartic of comp. jalap powder, which acted well. Partial paralysis of tongue and side of face continued for several weeks, double vision, showing in the beginning decided intercranial lesion. Child recovered without difficulty, and the mother finally, entirely, I believe.

I have observed other cases, but these will serve to illustrate in part the plan of treatment in the three stages of pregnancy. The following table from Bailly, modified to suit my views of treat-

ment, will more fully set forth the management to be adopted under the varied conditions :

CONFIRMED ECLAMPSIA.	{	1. Os completely closed.	{	A. Medical means.	{	Bleeding from the arm to the extent of 15 to 30 ounces at most; leeches to the mastoid processes. Calomel and Jalap. [I would give instead the Comp. Jalap powder.—G.] Cold applications to the head. Chloroform. [I would say chloral and morphine, one or both.—G.]
		(Pregnancy)		B. Surgical means.		If there is postponement of spontaneous labor, notwithstanding the cerebral disturbance, produce it artificially.
		2. Os incompletely dilated.				
		(Labor.)				
CURATIVE TREATMENT.	{	3. Os dilated or dilatable.	{		{	General rule: Wait to extract the child till the dilatation is spontaneously completed. Very exceptionally and only when the frequent and grave paroxysms resist medical treatment, complete the dilatation of the orifice with the hand or make incisions around the os for the purpose of terminating labor more promptly.
		4. Labor terminated.				If the contractions are strong, the child small, the labor rapid, and the paroxysms seldom, trust the delivery to nature; in the opposite conditions terminate it by turning, or with the forceps.
						Remove the placenta and clots without delay, then employ medical means.
ALBUMINURIA, ECLAMPSIA IMMINENT OR ONLY POSSIBLE. PREVENTIVE TREATMENT.	{	1. Pregnancy.	{	A. Medical means.	{	Moderate bleeding, [?] purgatives, sudorifics, baths, frictions, exercise, tonics, chalybeates.
				B. Surgical means.		Produce delivery (Tarnier) under certain conditions above mentioned. (Theoretical precept not yet settled by the facts.)
		2. Labor.				
						Trust it entirely to nature if its course is regular and rapid. Under opposite conditions terminate it by turning or by means of the forceps when the dilatation of the os will permit.

ARTICLE II.

A BRIEF REPORT OF ONE HUNDRED CASES OF RÖTHELN. By ROSWELL PARK, A.M., M.D., Chicago. (Read before the Illinois State Medical Society, May 17, 1881.)

It is not my purpose to treat in a general way of rötheln, but simply to speak of the peculiar features of an outbreak of the disorder in the Protestant Orphan Asylum of this city, of which I have professional charge. The disease had prevailed to a small extent in scattered localities through the city, and had been recognized as such, about a month before its appearance in the above institution. The first of my cases was diagnosed about the 8th of April. and during the five weeks following. ninety-five

children out of one hundred and forty, and two adults, manifested the usual symptoms of this exanthem. These, with a dozen or more cases in private practice, swell the number under my observation to more than a hundred.

Many circumstances rendered it difficult to make out a definite period of incubation, because I could not trace the contagion with sufficient reliability. One feature is worthy of attention first of all, and that is, that only two-thirds of the children exposed were attacked. This is analogous to what we see in the case of the other exanthems, scarlatina for instance; of three children exposed only two may take the disease.

Another point worth mentioning is, that most of these children had already had measles the previous winter or spring; for, during the early months of 1880, eighty-eight children in the same institution were attacked with morbilli.

There were no definite premonitory symptoms. Occasionally the patient would be languid or fretful for a few hours before the eruptive outbreak, and a very few coughed a little, but not in any way to attract particular attention. Along with the appearance of the eruption about twenty-five per cent. presented symptoms like those of the outset of measles. In ten per cent. of the whole number the bronchial irritation, conjunctival suffusion and general appearance were so characteristic that if these same children had not had measles to my certain knowledge, I should have been tempted to diagnose them as such.

The papillary eruption needs no description. I may say, however, that in three or four cases there was a slight tendency for it to become confluent. It appeared first on the neck, throat or behind the ears, traveling downwards, and was usually very distinct on the forearm and wrist. With the appearance of the first papules there was, in almost every case, so much flushing of the cheeks that any manifestation there was hidden from view. The same papules were also plainly visible in the roof of the mouth even before they were distinct on the surface.

Twenty per cent. of the cases had pharyngitis or tonsillitis, but in mild form. Fully one half of them had marked adenopathy in the cervical region or under the tongue.

While the pulse rate was rather high in some of my little

patients, the temperature was never any higher than seemed reasonable under the circumstances. Nausea and vomiting were exceedingly rare.

Not one of them suffered from any complication of importance during the progress of the r  theln. But undoubtedly they were left quite susceptible to dangerous sequel  . Four were prostrated with pneumonia in severe form, though all recovered; and several others suffered from more or less severe bronchitis or croup. This experience has taught me that there is necessity for no small amount of caution for some time after convalescence. In several cases I noticed a very slight furfuraceous desquamation.

The progress of the disorder was, in all cases, toward recovery, and with little or no treatment. I only advised medication when sympathetic fever ran high, when bronchial irritation was marked, or when the bowels were sluggish.

These cases are, I believe, fair samples of the disease which has visited our city, in an epidemic form, this past spring; and while individual cases have hitherto hardly been rare enough to be considered curiosities, yet the disease in its epidemic features has been new to the younger portion of the profession here. For this reason I am perhaps justified in reporting these cases without further apology.

1558 Wabash Ave., May 15, 1881.

ARTICLE III.

DIFFUSE IDIOPATHIC ANEURISM OF THE ANTERIOR TIBIAL ARTERY. TETANUS FOLLOWING AMPUTATION OF THIGH. By G. FRANK LYDSTON, M.D., late House Surgeon Charity Hospital; Resident Surgeon, State Emigrant Refuge and Hospital, Ward's Island, New York.

The patient, T. R., a painter by occupation, and thirty years of age, entered Charity Hospital, September 4, 1880. He gave a history of having had malaria and rheumatism, and several attacks of gonorrh  a, but said that he had been otherwise

healthy until four years since, when he contracted several sores upon his penis, which were termed "mixed sores," by his physician. In about eight weeks thereafter, he developed secondary syphilis, which was treated by a mercurial course with apparent success. Some time afterward however, he developed several tertiary ulcers upon his legs, for which he was treated in the hospital, and which healed readily in a few weeks. About eight weeks previous to last admission, several ulcers appeared upon the outer aspect of the left leg, just below the knee over the heads of the tibia and fibula. These were evidently syphilitic in character, and he was accordingly placed upon the "mixed treatment," with a large excess of the iodide of potassium. The benefit from this was only temporary, the ulcers soon beginning to extend, and the bone becoming involved. The odor from the ulcers soon became so offensive that it became necessary to transfer the case to the surgical pavilion. During December, six arterial hæmorrhages occurred from the central ulcer over the head of the tibia, which on several occasions were difficult to control. Necrosed bone was now plainly visible upon the heads of both tibia and fibula at the bottom of the ulcer. On December 10, a consultation was called. In the situation of the ulcer, there was now a large elastic tumor, and the finger being introduced into the ulcer, portions of blood-clot were detached, which seemed to be a portion of the tumor. Exploration decided that the tumor contained blood. During the examination, and while the limb was not under manipulation, severe arterial hæmorrhages occurred, rendering the application of a tourniquet necessary. The diagnosis of "diffuse aneurism" was made, and as the condition of the parts rendered deligation of the femoral unpromising, amputation of the thigh, in the lower third, was determined upon. On December 14 the operation was performed; Esmarch's "bloodless method," and the Lister spray and dressing being used. The amputation was made with lateral skin flaps, the femur being divided at about the middle of its lower third. The patient reacted from the anæsthetic very slowly, and the pulse being very feeble $\frac{5}{8}$ of brandy was given. The subsequent course of the case was as follows:

December 15. Temperature 103° F.; pulse 128. Patient feeling comfortable.

December 16. Temperature 101° ; pulse 100. Dressings removed and stump redressed under the carbolic spray. Stump looking well.

December 17. Temperature 101.4° ; pulse 116. Patient feeling well.

December 18. Temperature 101.5° ; pulse 110. Stump redressed and all stitches removed excepting two at upper angle of wound, on account of sloughing of the flaps to some extent.

December 19. Temperature 100.5° ; pulse 112.

December 20. Temperature 100° ; pulse 104. Stump redressed and dressings ordered to be renewed three times daily. Stump looks fair, but considerable suppuration going on.

December 21. Temperature 100° ; pulse 104. Patient quite comfortable.

December 22. Patient complained of a sense of depression, and expressed the fear "that something was going to happen to him," but he had no definite symptoms. Temperature 100° ; pulse 100.

December 23. Patient complained of "tingling and twitching sensations in the stump, and of some stiffness and soreness of jaws. Countenance anxious and pinched. Temperature 100° ; pulse 110. Was ordered sol. Magendie min. x, t. d.

December 24. Patient much worse. Trismus still more marked than on day previous. Some involuntary spasms of the extremities, notably of the affected thigh, have occurred since last visit. Countenance more pinched, and extremely sallow. Temperature 100° ; pulse 112. General appearance suggests a certain degree of septicæmia.

December 25. Several slight convulsions of limbs occurred during the night of the 24th. Jaws cannot now be opened sufficiently for the introduction of food. Fluids are expelled when drunk, and return partially through nostrils, suggesting paresis of soft palate. Any attempt to force open the jaws caused a slight general convulsion without loss of consciousness, and of short duration. Patient very feeble, and evidently sinking rapidly. Rectal alimentation and stimulation were resorted to, but to no purpose. Chloral hydrate was also administered per enema. Temperature 104° ; pulse 125. Death occurred on this date, 25th, at 11 P. M.

No severe muscular spasms occurred at any time, and there was no opisthotonos. The tetanoid symptoms were, I think, modified in great measure, by the septicaemic element which evidently existed. No autopsy was held, but an examination of the diseased limb was made shortly after its removal.

It was found that an ulceration and solution of continuity of the anterior tibial artery, about half an inch from its origin, had occurred. The eroded ends of the artery were found free, and connecting with a cavity about as large as a good sized orange, which was filled with disorganized blood-clot, occupying the inter-osseous space at the heads of the bones, the space between which was greatly enlarged from necrosis. This cavity had been formed by the pressure of its contained blood, the muscles having been displaced in its formation, and a pseudo sac formed by plastic deposit in the surrounding tissues. The degeneration of the artery was of considerable extent, there being some distance between the distal and proximal extremities in this situation, with portions of the degenerated arterial coats, present in the contents of the sac.

ARTICLE IV.

THE PATHOLOGY AND TREATMENT OF YELLOW FEVER; WITH SOME REMARKS UPON THE NATURE OF ITS CAUSE AND ITS PREVENTION. By H. D. Schmidt, M.D., New Orleans, La. (Continued from page 70, July No., 1881.)

GENERAL PATHOLOGY.

We have now studied the clinical phenomena of yellow fever, together with the pathological changes taking place in various tissues and organs during the course of the disease, and revealed by a post mortem macroscopical and microscopical examination; a comparison of these two sets of phenomena with each other, for the purpose of determining, as far as possible, the relationship necessarily existing between them must, therefore, be the next object in view. Formerly, when diseases were classified by certain groups of clinical phenomena, or symptoms, which they presented, it was chiefly these symptoms to which the physician directed his

attention, and by which he was guided in his treatment, consisting for the most part of remedies to which, in the cure of each particular disease, a specific action was attributed. With the rapid development of pathological science, however, the custom of treating disease in this empirical way was gradually abandoned for more rational methods, based upon a thorough knowledge of the intrinsic nature of the disease, obtained from the nature of the pathological changes observed after death in the tissues and organs. But even a knowledge of these changes, wrought upon the internal organs by the morbid process of one or the other disease, is insufficient to safely guide the physician in his efforts to obtain a cure, unless he is able to tell from the symptoms, presented by the particular condition of his patient, the exact and simultaneous condition of these organs; in other words, to form an accurate scientific diagnosis, the highest accomplishment of the practicing physician. In a considerable number of diseases, especially those of the heart and respiratory organs, medical science has reached this aim, and I am confident that, to a considerable extent, the same may also be accomplished in yellow fever.

In yellow fever, as in other acute infectious diseases, the most prominent phenomenon observed is that of fever: I shall, therefore, introduce my remarks on the general pathology of this disease with a brief consideration of the observed facts, relating to the *febrile process in general*.

Various theories on the phenomenon of fever have been advanced by medical men at all periods of medical history. The older theories were all based upon the principles of "humoral pathology," according to which not only fevers, but all other diseases depended upon a contamination of the blood, caused by the reception of noxious substances from without, or, in consequence of certain changes going on within the body, the febrile process being an effort of nature to expel the noxious matter. It was *Hoffmann*,* who first directed the attention from the fluids to the solid tissues, as the seat of the disease. He sought the source of fever in the nervous system, believing that it was orig-

* G. B. Wood, "Practice of Medicine," fourth edition, Vol. I, p. 101.

inally depending upon a spasm of the capillaries, and considering the heat of the skin and the arterial excitement which follow as the mere reaction of the system, necessary to overcome this spasm. A very similar theory was entertained and promulgated by *Cullen*, and, also, in a more or less modified form, by some other authors. Another theory of fever was, in the beginning of this century, put forth by *Clutterbuck*,* who regarded all forms as of local origin, and depending upon local inflammation. The so-called idiopathic fevers, therefore, he regarded as inflammation of the brain, designating them "encephalitis." *Broussais*, in announcing his doctrine of fever, agreed with *Clutterbuck* in denying the existence of essential fevers, but differed from him in ascribing all those forms, previously denominated idiopathic, to inflammation of the mucous membrane of the stomach, or that conjointly of the stomach and bowels; in other words, being *gastritis* or *gastro-enteritis*. The impression which *Broussais*'s theory had made, gradually faded, and there has been no doctrine of fever since which had a general prevalence, though the above theories of fever have, with various modifications, extended into our time. Thus, the neuro-pathological theory of *Hoffmann* and *Cullen* was, in a modified form, subsequently more developed by *Virchow*,† who regarded the phenomena of the febrile process, especially the elevation of temperature, as depending upon a depression (*Nachlass*) of certain parts of the nervous system, which he supposed to act as moderators of the production of heat. In still more recent times, however, the old theories based upon humoral pathology, appear to recover their lost grounds.

Although the most prominent symptom of fever, the abnormally increased heat of the body, has always been attributed to an increased waste of matter derived from the tissues, the discovery of a correspondingly increased formation of urea, found in the urine during the febrile process, and verifying this supposition, belongs to more recent times. The precise nature of the febrile process itself, and the true cause of the increase of bodily temperature, however, remained still unknown, and gave rise to

* L. c., page 103.† *R. Virchow*.—*Handbuch der Pathologie u. Therapie*, 1854, Vol. I, p. 33.

very numerous and laborious investigations, made by a considerable number of investigators, such as *Jochmann, Traube, Moos, Redenbacher, Uhle, Ringer, Brattler, Wingl, Wachsmuth, Warnecke, Huppert, Tschsschichim, Breuer, Chroback, Leyden, Ranke, Senator, Naunyn, Unruh*, and others.* A most able treatise, covering the whole subject, and embracing his own labors as well as those of others, was written by *Senator*, who, for his extended contributions to the knowledge of the febrile process, as far as it goes, may be regarded as a prominent authority. And as some of his special views seem to have been corroborated by still more recent observations, I shall not hesitate to extract from his treatise in the following discussion.

In the above mentioned investigations, the attention was particularly directed to the existing proportion between the production and discharge of heat, carbonic acid and water; and the most prominent facts thus far elicited were: an increased formation of urea in the urine, and an augmentation in the discharge of carbonic acid, water and heat.

The increased formation of urea is not depending upon a greater activity of the normal exchanges of matter in general, but rather upon a greater waste of albumen. The quantity of urea excreted during the whole fever amounts in the average to more than twice the quantity excreted during the absence of fever, but otherwise under the same conditions; that is, when the body is kept on fever diet. The average quantity of urea excreted on such diet, and taken by *Senator* as a basis, was 18 grms. in twenty-four hours.

The increase in the quantity of urea secreted begins, as was first observed by *Ringer, Traube, Joachmann, Uhle* and *Redenbacher*, before an elevation of the bodily temperature is noticed. *Senator*, who corroborated this fact by his experiments on dogs, in which, contrary to man, the activity of the urinary function increases during the febrile process, enlarges upon this phenomenon as follows: "Whilst, namely, with the dog the quantity of

* *Virchow u. Hirsch, Jahresberichte, etc., für die Jahre 1866-1872. Section: Allgemeine Pathologie.*

† *Senator*.—"Untersuchungen ueber den Fieberhaften Process, und seine Behandlung." Berlin, 1873.

urea discharged with the urine in about twenty-four hours may be looked upon as equal to that *formed* in the same time, such a presumption is not accepted as regards most febrile diseases in man. In the dog, at least in artificially produced pyæmia, the discharge of urea is favored by the augmentation of the water of the urine; and a loss of nitrogen by some other way than through the urine, as, for example, through the fæces, did not take place during the time of observation. With man it is different. Here, the amount of urine is usually diminished, and with this the chief source for the abstraction of nitrogen is limited. In consequence, the removal of the latter does not keep equal pace with the extensive disintegration of albumen; urea—or, as we may for the present suppose, bodies formed preliminary to urea—accumulate in the body, to be afterwards eliminated at a shorter or longer time after the defervescence of temperature, as the post-febrile augmentation of the discharge of the urea so frequently shows. Besides this, the last residue of oxydized nitrogen is certainly as yet not removed with the latter, but even in the succeeding days the origin of a portion of the discharged urea may still be referred to the time of fever. Moreover, the post-febrile augmentation of urea does not always show itself very conspicuously, as, quite frequently, the body rids itself of its surplus only very gradually, and an imperceptible transition from the febrile to the normal excretion of urea takes place, so that it is difficult to determine how far this is still depending upon the influence of the fever.” He further remarks that in different febrile diseases, much nitrogenous material, in various other forms than the ordinary constituents of urine, leaves the body, or, at least, is abstracted from the exchanges of matter, not only with the stools, but with the expectoration, or in the form of albumen in the urine, as it frequently occurs with high temperatures. These losses, which affect the organism only during the fever, but not when in a state of inanition, also bear in favor of a febrile augmentation of the exchange of albumen: for, if these quantities of nitrogen were not abstracted from the exchange of matter, the organism would discharge still more urea during the fever. According to *Hoppe*, the quantity of ammonia in the urine, also, is considerably augmented in febrile diseases forming an expendi-

ture of nitrogen not taken as yet into account in determining the quantity of urea. If, therefore, in the fever of the dog, the augmentation of the excretion of urea may be regarded as in just proportion to that of the disintegration of albumen, this is not applicable in the same degree to man. On the contrary, the disintegration of albumen during the fever of man is augmented in a greater measure, as can be calculated from the increase of the urea, and must, therefore, amount to more than double the exchange taking place under the same conditions without fever.

The conditions for the discharge of carbonic acid are, according to Senator, during the hot stage of the fever better than normal; under the most favorable circumstances, the discharge during the day-time is augmented from thirty to forty per cent., while during the night, it is probably, as in the normal condition, generally less. The formation of carbonic acid during fever, therefore, can, in the most favorable case, only augment to below thirty to forty per cent.

From the above facts, Senator concludes that in the fever of man no uniform augmentation of the whole exchange of matter, viz., the albumen and fat, takes place. As regards the disintegration of the constituents of the body during the febrile process, however, there are other facts known, which not only confirm it, but moreover indicate, which particular tissues are affected by the augmented disintegration. Above all, it is the exact investigations of *Salkowski* upon the excretion of the alkaline salts, which have shown that the quantity of potassa excreted by a healthy individual on fever diet, is from three to four, or even seven, times augmented during the febrile process, while a similar augmentation of soda does not take place. The latter, on the contrary, appears to be excreted in a smaller quantity, which is perhaps owing to the existing antagonism in the excretion of potassa and soda, shown by *Boecker* and *Reinson*, and more recently by *Bunge*. It is furthermore known that in febrile diseases the coloring matter of the urine considerably augments in quantity, which, according to *Vogel*, amounts to even more than four times the quantity excreted by healthy individuals on an ordinary full diet. No final product is augmented in the same measure as the urea, coloring matter of the urine, and the potassa,

not even the phosphoric acid; it is even doubtful, after the careful determinations made by *Rosenstein*, whether its excretion—in the urine—is much augmented under the influence of fever. As regards the excretion of sulphuric acid, the greater part of which is likewise derived from the combustion of albumen during the fever, nothing certain is known, probably because its secretion does not regularly and rapidly follow upon its formation.

Those nitrogenous tissues which are rich in potassa and hæmoglobin, the mother substance of the greater part of urea, are therefore, especially prone to disintegration, namely: The colored blood corpuscles first of all, and then the muscles, while the brain and spinal marrow, with the nervous tissues in general, though likewise rich in potassa, may be excluded, as they furnish no coloring matter of the urine, and also because the discharge of phosphoric acid is not augmented. Experience teaches besides, that the central parts of the nervous system are least affected by the febrile consumption, or by that caused by inanition. The fact, that it is the colored blood corpuscles which, first of all, suffer and disintegrate during the febrile process, is corroborated by the observations of *Koerber*, which showed that the decomposability of the hæmoglobin was increased in febrile diseases, and the statement of *Mannasseïn*, concerning the general diminution of the colored blood corpuscles in size during fever and with an elevated temperature. But if those corpuscles and the hæmoglobin are destroyed to a great extent; if, in consequence, the body becomes poorer in those elements which effect the reception of the oxygen from the air and its conveyance to the tissues, and if other elements do not take charge of this work, then the activity of the processes of combustion in the body must necessarily decrease in the same measure for want of oxygen. The further statement of *Mannasseïn*, that blood corpuscles rich in oxygen, increase in size, as also when under the influence of certain anti-febrile remedies—quinine, etc.—is a further proof that the function of the colored blood corpuscles is lowered during the fever; that the latter appropriate less oxygen, rapidly decrease in size and disintegrate.

Accordingly, the organism cannot absorb as much oxygen, and in consequence cannot oxydize as much material of the body to

its final products during fever, as it can without fever and under the same nutritive conditions.

If, now, the one final product, urea, is found in too large a quantity, already consuming a certain quantity of oxygen more than usual, it follows that the other final products, which require still more oxygen, must naturally be diminished in quantity; and from this, it again follows that the combustion of the non-nitrogenous constituents of the body—the fat—and, accordingly, also the formation of carbonic acid during fever, cannot be simply augmented. The increased destruction of the colored blood corpuscles, the carriers of oxygen, then, forms another cause for the granular and fatty disintegration of the tissues, taking place here in a manner similar to the effect of poisons which act by abstracting the oxygen. The conclusion, therefore, is that in the majority of febrile diseases of man, the body decomposes exclusively, or, at least, more than normally, nitrogenous material, *i. e.*, albumen, and in consequence becomes relatively richer in non-nitrogenous, *i. e.*, fat. Without oxydation, only water, increased in quantity, can be formed from the ultimate products of the exchange of matter—by synthesis and dishydration.

The quantity of urine in fever is usually in proportion to the supply of liquids, but its secretion is more unfavorable than in the normal condition, as a comparatively smaller portion of the received water is discharged by the urine than in the absence of fever, but otherwise under the same conditions. The quantity of the evaporated water is augmented during the fever, and amounts to even a little more than that of the expired carbonic acid; therefore, the quantity of water lost by evaporation is comparatively large. In what proportion the whole loss of water stands to its formation during the fever—with the exception of extraordinary losses—can as yet not be determined. With a large supply of water some of it may, as in the normal condition, be retained. The variations in the weight of the body during fever chiefly depend upon these unstable relations between the supply and loss of water.

In comparing the economy or exchange of matter during the febrile process, as above stated, with that taking place in an individual kept on a perfectly normal diet, Senator remarks that the

only two substances of excretion, from which the exchange of of albumen and fat may be calculated, are urea and carbonic acid, and that the daily normal excretion of the former amounts from 25 to 30 grms., while that of the latter is from 700 to 800 grms. This, based upon the values of combustion as determined by *Frankland*—4,263 units of heat for 1 grm. albumen, and 9.1 units for 1 grm. fat—corresponds to an exchange of 74 to 93 grms. albumen and 193 to 240 grms. fat, with a formation of heat of 2072 to 2580 units. The fever patients upon whom the investigations relating to the excretion of urea and carbonic acid were made, rather belonged to those who in their ordinary normal condition show a low equivalent weight of nitrogen, and whose daily excretion of urea deviates perhaps little from 25 grms. If these individuals discharge, at least during the first days and with a high fever, 40 to 50 grms. in 24 hours, then this is, in the most favorable case, a surplus expenditure of 25 grms. at the utmost in comparison with their normal condition, or a surplus exchange of 74 grms. albumen at the highest rate. In supposing that this albumen were completely oxydized, as in the healthy condition, to form urea, $C O^2$ and $H O^2$, an augmentation of heat, equivalent to 315 units, would result, or more still, because during the fever more albumen is disintegrated than corresponds to the urea.

In corroboration of the statements and views of Senator, but more especially for their own intrinsic value, I will recite some of the more recent investigations regarding the exchange of matter during fever and inanition, obtained by *Zuelzer*.^{*} They are as follows: “1. The total sum of the nitrogenous excretions is augmented during the state of febrile excitement, and diminished during the state of depression—inanition, convalescence. 2. The relative quantities of phosphoric and sulphuric acid—in proportion to the nitrogen—in the urine during the state of fever and hunger—do not exceed those found in muscle and brain. 3. In proportion to the nitrogen, more sulphuric acid is excreted in the state of fever and inanition, and less during convalescence than is excreted with a meat diet. Accordingly, these two final products, arising from the disintegration of the albuminous sub-

^{*}*Virchow u. Hirsch, Jahresbericht fuer das Jahr, 18* Vol. I., p. 220.

stances, are excreted through the urine in a relatively larger quantity than in the normal condition. During convalescence, however, the urine also contains—in a larger quantity than during inanition—nitrogenous substances, not derived from disintegrated flesh, but from the nervous substance, that is, from a tissue, in which the nitrogen is not associated with sulphur. Besides, the albuminous substances supplied for the reconstruction of the wasted constituents of the body, are mostly used in the body itself, while the secretion of bile is simultaneously augmented. 4. The relative quantity of the phosphoric acid is less during the fever than with a meat diet; during convalescence and inanition, however, it is larger, though smaller than on feeding with brain. During the fever, therefore, and simultaneous with the augmented disintegration of the nitrogenous tissues, phosphoric acid is retained in the organism to be excreted after the disappearance of the fever. In inanition—together with the diminished disintegration of the nitrogenous constituents of the body—the relative quantity of phosphoric acid in the urine is augmented. 5. Accordingly, the exchange of muscular substance, is augmented during the fever, whilst in inanition, as in convalescence, the exchange affects more the nervous tissue, which, in virtue of its richness in lecithin and cephalin (Thudichum) furnishes comparatively less nitrogen, but more phosphoric acid. 6. These processes in the nervous substance cannot be referred to a simple increase or decrease, as, during the state of inanition, according to the law of *Voit* concerning the loss of tissue during the state of hunger, the total mass of nervous substance experiences the least loss. The regular change from retention to augmented secretion of the phosphates under existing and depressing influences, as also the widely varying results of the elementary analysis of the brain, rather indicate that the quality of the constituents of the tissues may experience rapid and intense changes.”

The elevated temperature of the body in fever has usually been attributed to the increased disintegration or waste of tissue taking place during that process and liberating a surplus quantity of heat. The more recent investigations, cited above, however, have shown that the heat developed from this surplus exchange

of matter is not sufficient to account for that discharged from the surface of the patient, and that there must be other sources besides. The investigations of Leyden and Senator, particularly, have furthermore demonstrated that the discharge of heat during the whole course of the fever, though augmented, is nevertheless subject to considerable variations. The quantity of heat stands in no fixed proportion with the temperature of those accessible parts of the body, upon which it is usually measured, but may be less with a high temperature than with a lower, and even sink to the normal standard; in the stage of defervescence, especially when a critical sweat occurs, the discharge of heat is highest, amounting to double and triple the normal quantity. During the hot stage, it is augmented to one and a half, or nearly double the normal amount.

In order to understand these variations existing between the formation and discharge of heat, we need only examine the particular mode in which the healthy organism regulates these processes in keeping up the equilibrium of heat. As the heat, derived from the exchange of matter constantly occurring in every tissue of the body, is distributed throughout the latter by the circulating blood, it follows that the richer any part or organ is in blood, the higher will be its temperature; but as the amount of blood which an organ contains is proportionate to the number and caliber of its minute blood-vessels, the arterioles, venules and capillaries, its temperature will change with the contraction or dilatation of these vessels. In the normal condition, the tonus of these vessels, especially that of the arterioles possessing muscular fibers, is kept up by nerves—vaso-motor—connected with certain nerve-centers, chiefly placed in the middle portion of the medulla oblongata, but also in the spinal marrow; it is these centers, therefore, that regulate the normal temperature of the body by the amount of contraction or relaxation which they impose upon the muscular fibers in the walls of the vessels, or, in other words, by the amount of blood which they suffer to pass through the organ. The whole apparatus is reflex in its nature, and set and kept in operation by the impressions made, particularly upon the external surface of the skin; and it is thus that the application of cold will contract the minute vessels of the latter,

while moderate heat will cause them to become relaxed. To a certain extent, however, the discharge of heat from the body is also regulated by the lungs, the respirations increasing or diminishing in frequency; this is especially the case in animals clothed with a fur. Even in the normal condition, the quantity of heat produced within the body is not always the same, as, for instance, during the process of digestion, when by the more extended decomposition and combination of matter taking place, a surplus of heat is produced; or, also, during muscular exercise, when more muscular substance is disintegrated than in a condition of quietude and repose. With man, the limits of this capacity of regulating the temperature in the interior of the body are quite narrow, lying between 27° – 37° C. of the surrounding air; whatever is required beyond he has to replace by accessory means, as clothes, etc.

Now, as regards the discharge of heat during the febrile process, it is not augmented in the beginning or cold stage, but rather diminished; in the hot stage, or at the height of the fever, however, it is augmented 70–75 per cent. in the average during the day, and still more at the critical defervescence. As in the normal condition, the most heat is lost during the hot stage by conduction and radiation, while at the critical defervescence the discharge takes place by evaporation.

It has already been remarked that the augmented exchange of matter, indicated by the final products, urea and carbonic acid, does not account for the surplus of heat present in the body during the fever, and that there must be some other sources of heat besides. The most prominent of the latter are, according to Senator: 1. The consumption of those static forces, which, in the healthy organism, are stored up for the performance of any work. 2. The accumulation of heat during the pyrogenetic or cold stage of the fever. Besides these, other sources for the increased formation of heat may be sought in the greater disintegration and metamorphosis of albumen into urea, and in unknown processes concerning the formation of water.

With regard to the first of these sources, Senator presumes that the healthy body is in possession of latent forces, which are not generally liberated, but kept in store for extraordinary oppor-

tunities, such as an interrupted supply of nutriment, when they may be used, both for the performance of mechanical work, or for the development of heat. But as during the fever no external work of any import is done, these static forces are used up in the form of heat. Instead that in the healthy condition they are by the excitation of the will converted into mechanical work and heat, they will, on the other hand, under the influence of the febrile cause, be converted into heat only.

In the consumption of these static forces, which may either take place rapidly by mechanical work, or more slowly by hunger, carbonic acid is developed by the combustion of certain non-nitrogenous substances, containing these forces. The same should be the case, if the consumption of the latter takes place during the fever, to form a source for an augmented formation of carbonic acid during this process. When, nevertheless, such a source cannot be proved, or is not very conspicuous, even if present, no objection can be raised against the presumption that these static forces are liberated. For the surplus of carbonic acid formed here may be counterbalanced on the other side by being formed in a smaller quantity in such a manner that the albumen is not, as in the normal condition, completely consumed, but that it leaves behind, after the splitting off of the urea, a non-nitrogenous rest, more or less related to fat—the fatty degeneration of the parenchymæ in severe febrile diseases corroborates this hypothesis. The difference between the exchange of matter in a healthy and in a fevered individual is as follows: The healthy individual converts, under ordinary conditions, daily, a certain quantity of albumen and fat into the corresponding quantity of urea and carbonic acid, and replaces the loss by an equal quantity of fresh supply, keeping thus his store of static forces at the same level. If the fresh supply is insufficient, that is, during a more or less perfect state of hunger, then he consumes, in proportion to his state of nutrition, a quantity of albumen and fat of his body, and his store of static forces decreases. But the combustion is always perfect, that is, the quantity of albumen and fat destined to the exchange is completely converted into urea, carbonic acid and water. During work, he consumes the same quantity of albumen, with the only difference, that the static

forces, which during the state of hunger are more gradually consumed, are here liberated in a short time, giving rise to a rapid development of carbonic acid, which otherwise would have been discharged very gradually. During the fever, a larger quantity of albumen is disintegrated as during inanition, but only to urea and fat. The static forces are, perhaps in consequence of the enormous destruction of albumen, more rapidly consumed, and, for this reason, more heat and carbonic acid result in an equal time from this consumption than in a state of hunger; the carbonic acid, however, which would have resulted from the complete combustion of the albumen, is left out, so that, on the whole, nothing, or only very little more than in the corresponding time of hunger, is formed.

As regards the accumulation of heat during the cold stage, it is presumed that it really occurs. Even the normal discharge of heat during this stage would result in a saving or accumulation of it, because the changes in the exchange of matter are already taking place previously to the commencement of the chill, and as conditions for an augmented formation of heat may be found, besides, in the phenomena accompanying the chill itself. This presumption is moreover corroborated by the commencing augmentation of the discharge of urea, and the already preceding sensation of weakness and depression—phenomena which most probably depend upon the augmentation of the disintegration of albumen just beginning, and the consumption of the static forces in store. The spasmodic contraction of the blood vessels of the skin, together with that of the smooth muscular fibers, causing the “goose skin,” and even of voluntary muscles—causing the chattering of the teeth,—must give rise to a development of heat. In the beginning of febrile diseases, there is always a period to be found in which the morbid changes, the elevation of the interior temperature, the augmented secretion of urea and other changes in the urine, the increased frequency of the pulse and of respiration, and, finally, various subjective sufferings, show the existence of the febrile process, and with it the augmented formation of heat, even in the absence of the normal heat of the skin, or without an augmentation in its discharge; a period, therefore, in which an accumulation of heat may be presumed to take place.

The variations in the quantity of heat discharged, occurring during the fever, do not depend upon a total loss of the capacity of the skin to regulate the temperature of the body, but are rather owing to an abnormal excitability and irritation of the cutaneous vessels, brought about by the influence of the original cause of the fever, and probably reflected to the respective nerve-centers. It is the irregular and unequal contraction of these vessels, therefore, which prevent the equalization of the surplus of heat present. This irregularity of contraction and dilatation of these vessels during the febrile process has been satisfactorily proved by Senator and other investigators, who carefully observed it on the vessels of the ears of fevered rabbits and dogs; and it explains the alternate sensations of heat and cold, experienced by fever patients, especially before the fever is completely established.

From the above statements, concerning the elevation of temperature during the febrile process, it may be concluded that it is brought about by a disproportion between the abnormally augmented formation and discharge of heat; though the discharge at the height of the fever may be greater than normal, and at times even greater than the febrile formation of heat. The disproportion, therefore, is not equally prominent in each phase of the fever, and it is supposed that every hot stage has been preceded by a pyrogenetic, or cold stage, for the accumulation of the heat, just as, on the other hand, it terminates by a stage of defervescence with an augmented discharge of heat. In mild cases of fever, the same variations occur in the discharge of heat, only more gradually and less intense.

Of late years, numerous additional investigations, regarding the febrile process have been made, chiefly for the purpose of determining the exact relation existing between the central and peripheral temperature, and, also, of the relative temperature of different parts of the external surface of the body itself, as, for instance, that of the right or left side of the thorax during febrile diseases of the respiratory organs. In these investigations, the temperature of the axilla, or, in some instances, of the rectum, was taken as a standard of the internal heat, while the skin between the toes, or other parts of the surface were chosen

for measuring the external temperature. But, though the results of these investigations, in general, corroborated the statements of Senator, and with it the neuro-pathological theory of Virchow, some contradictory statements have not been wanting. Thus, *Murri*,* basing his views upon the results of numerous experiments on animals, denied any dependence of the febrile process from the nervous system as chief regulator of the animal heat, and attempted to establish a bio-chemical hypothesis of fever in place of the neuro-pathological, now prevailing. He especially asserted that, in most cases of fever, no abnormal proportion between the peripheral and central temperature could be shown, and that the difference between these temperatures were neither more variable, nor much greater than in the normal condition; but, on the contrary, often, and for a long time, remained unchanged during the course of the disease; frequently, even, the surface would be warmer than the central organs. *Jacobson*,† who experimented on a number of fever cases of pericarditis, pleuritis, typhoid fever, pneumonia, articular rheumatism, and tertiary intermittent fever, made use of a thermo-electrical apparatus. For the determination of the peripheral temperature, he introduced electrodes, in the form of fine needles, beneath the upper layer of the epidermis upon different parts of the cutaneous surface; for the central temperature the axilla was chosen. The results of these examinations were very inconstant. While, at one time, the difference of temperature between the axilla and the upper layers of the cutis was less during the fever than during the apyrexia, it would be greater at other times. Thus it varied irregularly between different places of the skin in the same individual, for, while the thermometer showed one and the same degree of temperature in the axilla, that of the skin was observed to undergo very considerable variations. The difference between the central and peripheral temperature was found to be in no way less than during the apyrexia. The same results were obtained in measuring the comparative temperature of the mouth and skin. These observations showed that at the acme of the hot stage, also, an alternate contraction and dilatation of the

* L. c. für das Jahr 1875, Vol. I, p. 281.

† L. c.

cutaneous blood-vessels takes place, and that during this stage, the quantity of blood in these vessels at the periphery of the body, and, in consequence, also the discharge of heat, are subject to variations within extended limits not only at different times, but simultaneously on different places. *Schuelein*,* with the view of answering the same question, instituted a series of thermometrical examinations on fever patients, for which he had availed himself of maximal thermometers with very small cylinders, corresponding very accurately with each other, and of which the one was introduced in the closed axilla, while the other was put between the first and second toe in such a manner as to completely embrace the bulb containing the mercury. The patients were quietly lying in bed under a light cover; the temperature of the room was almost always the same, varying with some exceptions, between 18.0° and 20.0° C. In healthy individuals, it was found that while the temperature of the axilla remained nearly constant, that between the toes showed great variations. In the course of typhoid fever, peritonitis, acute articular rheumatism, erysipelas, endometritis, miliary tuberculosis, and cheesy pneumonia, continuous variations of temperature of the skin, not corresponding with those of the axilla, took place. In typhoid fever, they were observed within considerably wide limits, even when measured every quarter of an hour. During a severe frost a descent of the temperature of the skin nearly coincided with a rise of that in the axilla. Another series of observations, the results of which corroborated the statements of Jacobson and Schuelein, were made by *Schuck*.† *Wegscheider*,‡ who made his examinations on pneumonia and typhous patients, states, that the temperature of the periphery was subject to great variations, not running in any way parallel with those of the axilla. They were greater during the fever than in the feverless condition, a fact which he regards as depending upon an abnormal state of excitation of the cutaneous vessels.

A number of other observations have been made on this subject, which it is needless to cite, as they likewise corroborate the

* L. c.

† L. c. für das Jahr 1877, Vol. I, p. 219.

‡ L. c.

variations occurring in the contraction and dilatation of the cutaneous vessels during the febrile process.

In reviewing the preceding sketch of the febrile process, it will be found that though the chemical and physical processes concerned were satisfactorily explained, nothing was said which could throw any light upon the primary cause inducing the abnormal phenomena characteristic of fever. But as, in order to form a correct idea of the general pathology of yellow fever, it is important to have a clear understanding, not only of the chemical and physical processes concerned in the febrile process in general, but also of the principal views which at the present time are entertained as to its primary cause, a brief discussion on the subject is demanded.

Judging from what is known about the chemistry and physics of the febrile process, as demonstrated in the preceding sketch, the most prominent phenomenon of the whole is the over-production of heat, depending chiefly, but not entirely, upon an increased disintegration and exchange of matter. This heat, however, though its formation may from the beginning of the fever be going on at an equal rate, is not equally distributed throughout and discharged from the body. In consequence, the elevated temperature in fever, as once said before, is brought about by a disproportion between the abnormally augmented formation and the not in the same degree augmented discharge of heat, though the discharge at the height of the fever may be greater than normal, and at times even greater than the febrile formation of it. But, while the chief cause of the latter is the augmented disintegration and exchange of matter, the cause of the unequal discharge of heat, depending upon an abnormal and irregular contraction of the cutaneous blood-vessels, must be looked for in the nervous system. The question to be answered in relation to the primary cause of fever, therefore, is whether the increased waste of matter in the organism is the result of the accumulation of the normal quantity of heat within the body, depending upon the diminished discharge from the surface, on account of the irregular and abnormal contraction of the blood-vessels of the skin,—or whether the augmented disintegration of matter, especially the albumen, preceded the contraction of these

vessels, and by some irritative influence upon the respective nerve-centers had perhaps, itself, been the cause of the contraction. In the one case, the process would have commenced in the nervous system, and be in concert with the neuro-pathological theory; in the other, its starting point would have been the blood, and it would correspond to the principles of humoral pathology. In examining the subject a little closer, it will be found that neither the one nor the other theory can be strictly and exclusively applied to every form of fever. For, in taking, for example, the febrile process as it accompanies the so-called infectious diseases which are supposed to depend upon a certain poisonous material entering the blood through the avenues of the lungs and the alimentary canal, it will be obvious that the process commences in the domains of humoral pathology; while, on the other hand, the febrile process following the shock of a severe bodily injury, and representing the so-called symptomatic fever, most probably took its start from the nervous system. Therefore, the primary cause of some fevers will be strictly humoral, whilst that of others will be nervous. But it must be remembered, that though in a number of fevers the effect of the cause, representing some noxious matter, is directly made upon the blood, it may also, through the medium of this liquid, very rapidly extend to the nervous system, which, reacting upon the one or other organ of the body, may induce serious pathological changes. And, in the reverse, in those fevers associated with severe surgical injuries, the disturbance and derangement in the nutrition of the injured parts may give rise to a certain pathological condition of the blood, which, through the medium of the nervous system, may conjure up the febrile process. We see, therefore, that the febrile process in general cannot be made exclusively to fit one or the other theory, but being of a complex nature, remains independent.

The neuro-pathological theory of *Virchow*, applied to the febrile process, explains many of the phenomena manifested, as, for instance, that of the irregular discharge of heat from the cutaneous surface by an irregular spasmodic contraction and dilatation of the cutaneous blood vessels, caused by a morbid irritation of the vaso-motor nerves. But, although the phenomenon of the

non-equalization of heat is of a nervous origin, it does not necessarily follow that the augmented disintegration and exchange of matter is likewise the direct result of a perverted nervous action, for it has, as yet, not been positively shown that the original production of animal heat depends solely upon the nervous system, unless we presume the existence of special nerve-centers performing the function of regulating the exchange of matter, and with it the production of heat. The experiments of *Magendie*, *Bernard*, *Buettner*, *Rollet*, *Mantigazza*, *Schiff*, *Vulpian*, *Obolensky*, *Haidenhain* and *Legros*, made on animals in relation to this subject, and the clinical observations of *Samuel*, *Weir*, *Mitchell*, *Morehouse*, *Keen*, *Charcot*, *Erb*, and others, on man, however, seem to indicate the existence of so-called "trophic" nerve-centers, which regulate the nutrition of the tissues and organs, though on the other hand, a number of other investigators have obtained negative results from their experiments. The decrease of heat in paralyzed limbs, also, points to the influence of the nervous system upon the exchange of matter in the paralyzed parts. A number of experiments, consisting in the section of the spinal marrow, have likewise been made on animals by *Bezots*, *Ludwig*, *Thiry*, *Tscheschichin*, *Schiff*, and others, for the purpose of determining the variations of temperature in the extremities, with other observations on cases of severe injuries of the spinal marrow of man; these, however, may be passed over, as the results obtained are to a certain extent contradictory, and only seem to indicate local disturbances in the function of the blood vessels.

Now, even in recognizing the doctrine of trophic nerve-centers and nerves, and in admitting that the exchange of matter in the organism, constantly taking place in the fluids and solids, depend upon nervous stimuli, a special cause will still be required to act as such, and, accordingly, it may be presumed that in a similar manner as the nerve-centers presiding over the circulatory and respiratory functions, are stimulated to their rythmical reflex actions by the relative quantity of oxygen and carbonic acid in the blood, the normal combinations and decombinations of matter, constantly taking place in the blood and tissues, may form a stimulus for the trophic centers. As long as the organism remains

undisturbed by external injuries, or by the influence of noxious substances, introduced into the blood from without, the trophic centers, then, will discharge their function in the same regular reflex manner as the respiratory; the exchanges of matter will take place in their just proportion, *i. e.*, each atom of matter will be replaced by an atom of fresh matter, and the normal nutrition of the tissue will be preserved. At the same time, the quantity of heat liberated by the exchange of matter will be counter-balanced by an equal discharge, regulated by the normal contraction and dilatation of the cutaneous vessels. But, as soon as any foreign substance, that is, a body incapable of being assimilated by the organism for the rejuvenation of the tissues, and therefore incapable of becoming a constituent of the latter, enters the blood, it will be removed by one or the other of the secretory organs, the only outlets from the organism. There are a large number of substances, some of which are used as medicines, which thus may enter and leave the organism without injury, and be detected in the secretion, while there are others, which, after entering, will, either catalytically, or by entering into combination with the organic constituents of the blood and tissues, exert a noxious or fatal influence upon it. To these belong the so-called infectious poisons with which we have to deal in this treatise.

In the symptomatic fevers, the morbid impression may be carried to the vaso-motor or trophic nerve-centers by the sensory nerves of the skin, or of the traumatic lesion, affecting them in the same manner as the respiratory centers are affected by sudden impressions made upon the skin, etc.; in the fever of infectious diseases, however, experience teaches that they are affected through the medium of the blood. Even Virchow, when founding the neuro-pathological theory, admitted this probability in saying: * “The rapidity with which febrile phenomena appear and disappear, likewise points to a cause residing in the nervous system, though the noxious material, affecting the nervous system, may be sought in the blood.” And, further on: “In the study of the chemical nutritive processes of the body, it is of advantage to exclude the nerves as long as possible, as it has been shown that

* Virchow.—Handbuch der Speziellen Pathologie und Therapie, 1854, Vol. I, p. 36.

they exert no other influence, but in determining quantity or, acting as excitors or moderators, for all qualitative changes come from other directions."

In studying the general pathology of yellow fever, we must keep in mind that the clinical phenomena exhibited during its course, and the pathological changes met with after death, *do not*, as I have hinted at before, *exclusively* belong to this disease, but are equally observed, though in a more or less modified form, in other infectious, and even non-infectious diseases. It is only when they are considered as a totality, that they may represent the characteristic features of the disease. Thus, the continuous type of the febrile process witnessed in yellow fever, together with the congestion and parenchymatous infiltration and degeneration in various organs, are equally observed in other infectious diseases, particularly those of a contagious nature. The chief difference seems to lie in the specificity of the noxious poison, and in its tendency to affect particular organs in the different diseases, while its general effects upon the whole organism in these affections bear much resemblance to each other.

In the beginning of this treatise, I have already remarked that, in all cases of yellow fever, as well as of other infectious diseases, there is a prodromal stage, during which the poison exerts its noxious influence upon the blood, and primarily deranges the normal exchanges of matter. The symptoms of this stage, consisting, as we have seen, in a general discomfort, anorexia, lassitude, headache, etc., are decidedly nervous in character, from which fact it may be presumed that it is the nervous apparatus, which, first of all others, experiences the effects of the noxious poison. Now, although it is the blood with which the poison comes first into contact, it remains nevertheless difficult to determine whether it is directly carried by this fluid to the tissues of the nervous apparatus to make a direct impression upon them, or whether, in one way or other, it first contaminates the plasma of the blood, and, accordingly, makes an indirect impression upon the protoplasm of these tissues by deranging their normal nutrition. The latter supposition appears to be the most probable. At any rate, the influence which the poison at this period of the disease exerts upon the nervous system, is depressive in its nature, and

appears to be directed to the vaso-motor centers. The immediate effect of a depression of these centers is a relaxation of the muscular elements in the walls of the blood vessels, especially of the smaller arteries, followed by a dilatation of these vessels and a hyperæmic condition of a moderate degree, upon which the prodromal phenomena seem to depend. Judging from the degree of the headache, it appears that the hyperæmia of the brain, upon which it depends, is at this period principally confined to the pia-mater, though the minute vessels of the cortex cerebri, also, may be slightly over-filled with blood. The sympathetic ganglia likewise suffer from the effects of the poison, and, moreover, exert their depressing influence upon the functions of the liver, stomach and kidneys. The degree of the prodromal symptoms in the different cases, is obviously proportionate to the quantity and intensity of the poison absorbed, but also to the particular susceptibility to the noxious influence of the poison by the person infected. In persons very susceptible to the absorption and influence of the poison, and who are exposed to it in its intense form by proximity to a severe case of yellow fever, the prodromal stage, therefore, may be comparatively short, while in others, not as susceptible, and exposed to the poison in a milder form, the prodromal phenomena may be milder, and extended over a longer period of time. From this difference, observed in the severity and length of the prodromal stage in different cases, we may presume that the abnormal disintegration and exchange of matter in the organism, resulting from the effects of the poison, must have proceeded to a certain extent, before the febrile process can start into action. The augmented quantity of urea, found in the urine before an elevation of the normal temperature of the body can be perceived, shows the correctness of this supposition. But, while the first impression of the poison upon the nervous system is of a depressive character, a sudden change takes place with the true commencement of the febrile process, evidently owing to a certain reactive effort of the vaso-motor nerve-centers, and manifesting itself in the abnormal excitability and irritation of the cutaneous blood vessels. It is impossible to decide whether this irritation proceeds from the vaso-motor nerves of these vessels, or from the sensory nerves of the skin, to be propagated

to their respective nerve-centers, thus constituting a simple reflex action; or, whether it originates in the latter themselves, and is originally caused by the now concentrated effects of the infectious poison, or by the products arising from the augmented disintegration of albuminous substances, accumulated within the blood, and increasing the previously commenced disturbance of nutrition. The first effect of the augmented excitability of the vaso-motor nervous apparatus, then, is observed in the contraction of the cutaneous blood vessels, giving rise to the phenomena of the pyro-genetic stage, the mildness, severity, or duration of which evidently depends upon the quantity and intensity of the fever poison in the blood. Although those initiatory symptoms of the febrile process are subject to great variations in different cases of the disease, I do not think that they are ever entirely absent, but that, even in those cases in which no regular chill is observed, an alternate sensation of cold and heat, indicating the irritation of the respective nerve-centers, is experienced by the patient,—while in others, in which a regular chill, or even rigor, occurs, the spasmodic contraction of these blood vessels is more decidedly pronounced. As soon as the nervous energy, causing the contraction, is exhausted, a relaxation of the muscular fibers in the walls of the blood-vessels, followed by a dilatation, takes place, and in consequence of a larger quantity of blood coming from the central and warmer parts of the body, passing now through the vessels, the temperature of the skin gradually rises, until the hot stage of the fever is fully established.

In considering the disturbance of the nervous functions in yellow fever, we must not imagine that a depression or excitation of the nervous tissues takes place simultaneously throughout all parts of the nervous system, and that in consequence the blood vessels of all organs must be in a corresponding state of dilatation or contraction. On the contrary, as will be seen, the vessels of one part of the body may be relaxed, while those of another may have preserved their normal tonus, or, as in the case of the cutaneous vessels, may alternately be contracted or relaxed. In order to appreciate correctly the relative disturbances in the functions of the various organs of the body, and the pathological changes taking place in the blood and tissues during the whole course of

an infectious disease like yellow fever, one should be familiar with all the anatomical and physiological minute details of the entire organism, the study of which, alas! is rather neglected by the majority of practicing physicians. Without the knowledge of these details, which, after all, constitute the basis and greater portion of true medical science, the physician can only form an indefinite, vague idea of the normal and pathological processes taking place in the human organism, while, with it, he may represent to his mind the entire mechanism of the body in all its details, and in full operation.

Already, before the commencement of the hot stage, pains in the joints and limbs, as we have seen, are added to those in the head, increasing in severity with the rise of the temperature, until the fever is fully established, when they either disappear, or continue in a milder form during the hot stage. These pains, which are most severe in the lumbar region and pelvis, must probably depend, as mentioned once before, upon the congested condition of the blood vessels of the pia mater, and of the spinal marrow in the lumbar region, whence they are reflected to the joints and muscles affected. Whether these pains, as well as those in the head, are simply owing to the pressure of the congested blood vessels upon the nervous substance, or whether their severity partially depends upon the direct noxious influence of the infectious poison upon the nervous tissue of the affected parts, I will not venture to decide, though the latter supposition appears probable from the fact, that, in fatal cases, these pains generally disappear before death, while the blood vessels are still found congested with blood corpuscles after death.

The nervous apparatus, through the influence of which the circulation of the blood is sustained and regulated, is quite complex, and consists of several nerve-centers. While the rhythmical movements of the heart, by which the blood is kept in circulation, are most probably caused by the influence of certain nervous ganglia situated in the muscular substance of the heart itself, they are regulated by other centers situated in the medulla oblongata, and operating through the pneumogastric and sympathetic nerves: the nerves which the heart receives from the cardiac plexus are derived from these sources. The fibers of these nerves, however,

differ very considerably in their function, for while the pneumogastric fibers transmit only inhibitory stimuli, the other transmit stimuli which accelerate the heart's action. Accordingly, an irritation of the fibers of the pneumogastric nerves will be followed by a retardation, or even arrest of the movements of this organ, while an irritation of the centers in the medulla oblongata will produce an acceleration, as long as their communication with the heart through the spinal marrow, rami communicantes, first thoracic ganglion, etc., is not interrupted. An acceleration of the heart's action will also be produced by an increased pressure of the blood within the cavities of the organ, as may be caused, for example, by a contraction of the smaller arteries, depending upon an irritation of the vaso-motor centers, likewise situated in the medulla oblongata. From this it is easy to understand how an irritation of the medulla oblongata, produced by the noxious influence of the yellow-fever poison, will give rise to one of the most prominent phenomena of the febrile process, the frequency of the pulse. Besides this, the elevation of temperature also, depending upon the augmented disintegration and exchange of matter, initiating the febrile process, forms another cause for the accelerated action of the heart. The latter assertion, however, must appear contradictory to the circumstances, before mentioned, of the falling of the pulse on the second day, a time when the temperature is still rising. But may not this phenomenon be owing to an irritation of the inhibitory centers of the pneumogastric nerves, set up at this time?

The abnormally augmented disintegration and exchanges of matter in the organism, originally caused by the presence of the infectious poison in the blood, and giving rise to an increased formation of urea and other products, as well as to an augmented quantity of heat, will now continue, until the poison is destroyed, or eliminated from the system, while, at the same time the products arising from these abnormal processes, or the direct action of the poison itself upon the nervous tissues, or upon the morphological elements of the blood, may give rise to the abnormal excitability and irritation of the involved nerve-centers with their nerves.

The morbid excitability of the nervous tissues, however, does not appear to be confined to the centers and nerves already mentioned, but, moreover, extends to other parts of the nervous system, giving rise to other phenomena of the hot stage, consisting in the congestion and deranged secretion of several organs. Thus, the congestion of the blood vessels of the conjunctiva very likely depends upon a neuro-paralysis of the vaso-motor nerves, and is similar in character, though inferior in degree, to that produced in the blood vessels of the rabbit's ear by the well known experiments of Claude Bernard of dividing the sympathetic nerve in the cervical region. The deeper seated pains of the eye, and also the slight photophobia, are phenomena which depend upon the irritation of the trigeminal nerves and their centers. In the same manner, the congestion and irritation of the mucous membrane of the pharynx, œsophagus, and stomach are explained; for in considering the fact that all the minute blood vessels of the brain, but particularly those of the pons varolii, corpora quadrigemina, and medulla oblongata, are found after death in a congested condition, completely filled with blood corpuscles, there will be no difficulty in understanding the congestion and irritation of these mucous membranes, if we remember that the nuclei, or centers, of the trigeminal, glosso-pharyngeal and pneumogastric nerves, supplying these parts, are situated near each other, alongside of the aqueduct of Sylvius and the floor of the fourth ventricle of the brain, and that all these nerves closely communicate with the sympathetic. Judging from the scarlet-red color of the congested vessels of the conjunctiva, gums and edges of the tongue, during the first part of the hot stage, the congestion is arterial in its nature, that is, depending upon a relaxation of the arterioles and the ensuing afflux of blood. As regards the stomach, it hardly needs mentioning that the nausea and inclination to vomiting are due to the irritation of its mucous membrane, the secretory function of which, however, is not suspended, but only deranged, as shown by the quantity of frothy mucoid liquid thrown up at this time. As regards the liver, kidneys and supra-renal bodies, I have already mentioned, when discussing the pathological anatomy of these organs, that the parenchymatous degenerations, observed after death, have been initiated by a

congestion of the blood vessels, depending most probably upon the same causes as that of the mucous membranes above discussed. But, before passing to the discussion of these parenchymatous changes, I must make some remarks on the condition of the blood.

In the section of this treatise devoted to the pathological anatomy of the blood and a number of organs, I showed that, besides an unusual tendency of assuming the mulberry or thorn-apple forms, manifested by the colored blood corpuscles, nothing abnormal, or foreign, could be detected in this fluid, when carefully examined directly after it had been removed from the living patient; while, on the other hand, in almost every organ in which a congestion had occurred, numerous extravasations or infiltrations of the coloring matter of the blood corpuscles, the hæmoglobin, into the parenchyma were met with. The question arises, therefore, whether the hæmoglobin parted from the colored blood corpuscles only in those places where the extravasations occurred, by virtue of the existing retardation of the circulation in the congested capillary vessels, or whether it escaped from the corpuscles while they were still in an active circulation. In answering this question, it may be said, that the direct cause of these extravasations evidently was the retarded circulation, but that there also existed a remote cause, consisting in a want of coherence between the hæmoglobin and the protoplasm of the blood corpuscles, facilitating, as soon as the retardation took place, the escape of the former from the latter. And, it may further be presumed, that not all the hæmoglobin escaping from the blood corpuscles, passed through the walls of the vessels, but that perhaps the greater portion became mixed with the liquor sanguinis and was carried into the general circulation, giving rise to the jaundice. Thus, it will be seen, *that the icterus in yellow fever is not owing to the presence of bile in the blood*, as is believed by a large number of physicians, *but to the presence of free hæmoglobin*, and represents in truth the so-called "*hematogenous*" jaundice. A "*hepatogenous*" jaundice cannot take place, as the larger as well as the smaller hepatic ducts are found perfectly open, and as the secretion of bile during the disease, in the majority of cases, is rather diminished, or even suspended.

As regards the presence of free hæmoglobin in the blood, however, some doubt might still be expressed on account of its not having been demonstrated by the microscopical examination of so many specimens of fresh blood, taken from the living patients. The reason for this failure is, that the quantity of hæmoglobin contained in the exceedingly thin layer of blood required by microscopical examination, is too small to exhibit its yellow color to the eye of the observer, and that in order to be seen, the blood should be saturated with the coloring material to such a degree as would require a quantity larger than would be consistent with life. Free hæmoglobin, therefore, may be present in the circulating blood without being detected by the microscope. The faintly yellowish tint, appearing, in a number of cases, as early as the third day of the disease, in the conjunctiva and the skin, corroborates this assertion; though, in the majority of cases, the jaundice appears later, if it appears at all during life. The yellowish tint of the conjunctiva and skin, however, is not produced by the free hæmoglobin still contained in the circulating blood, as its presence is not perceived until it has escaped from this fluid into the juices of the surrounding tissues, to be absorbed by the cells of the epithelium of the conjunctiva, or those forming the lowermost stratum of the epidermis of the skin.

The extravasations of hæmoglobin into the parenchyma of various organs, which I have observed to occur in yellow fever, fully corroborate the generally accepted fact mentioned before, that in the febrile process, it is these nitrogenous tissues, rich in potassa and hæmoglobin, especially the colored blood corpuscles and muscles, which are most prone to disintegration, and which first suffer from the effect of the poison.

Let us now direct our attention to the liver. Although in most cases of yellow fever, as I have shown, the traces of congestion of the blood vessels, observed after death in this organ, are not as conspicuous as in other organs, there remains for the reason already given, no doubt but that the liver is one of the first organs which experience the deleterious effects of the poison, and participates in the general hyperæmia resulting from the depression of the vaso-motor nerves, manifested during the prodromal stage. But, like in the stomach, as soon as reaction

takes place, the secretion of bile is resumed, and, very likely, for a short time abnormally increased, as may be inferred from the vomiting of bilious matters, observed in a number of cases during the commencement of the disease. Judging from the fact that most cases of miasmatic fevers, even the simple intermittent, are accompanied by some derangement of the biliary functions, I have always been inclined to regard the liver as that organ through which the organism makes the first attempt at eliminating the miasmatic poison by stimulating its secretory function. The same may take place in yellow fever, as above mentioned, though the abnormally stimulated secretory function will soon be exhausted, and an opposite condition prevail. But, besides this stimulation through the secretory nerves, it appears very probable that the liver receives another stimulus, proceeding from the part of the blood, for the purpose of removing, and converting into bilirubin, the free hæmoglobin escaped from the colored blood corpuscles. But like even in the normal condition, the amount of labor which the parenchyma of a gland can perform, stands in a certain proportion to the quantity or number of its secreting cells, only a small portion of the free hæmoglobin can be converted into bilirubin by the liver, while the rest extravasates through the capillaries of the different organs and tissues as we have seen, to be absorbed by the surrounding cells, or to mingle with the fluids of the tissues. Thus, the abnormal stimulation of this organ, accompanied by the increasing disturbances of the nutrition of the whole organism, soon leads to a state of exhaustion.

But, while these functional changes are taking place in the liver, the abnormal processes in the blood, relating to the augmented disintegration of the albuminous constituents, and their metamorphosis into the final products urea, carbonic acid and water, are likewise running their course. The albumen, thus undergoing disintegration, may be derived from two sources, *i. e.*, from the circulating albumen of the blood, designated by *Voit* the "store albumen," or from the albumen of the tissues, the "stable" or "tissue-albumen." As the former, on account of the small quantity, is soon consumed, it is especially the latter which suffers the greatest losses during the febrile process. In consequence of the augmented formation of urea, consuming consider-

ably more oxygen than is normal, the other final products, requiring still more oxygen for their formation, must fall short in quantity, and the combustion of the non-nitrogenous constituents be retarded. But, as an abnormal quantity of albumen is decomposed without a corresponding quantity of non-nitrogenous material, such as fat, the latter accumulates in the blood, and gives rise to the *fatty infiltration* of the different organs, already described.

Fatty infiltration of the liver, besides being met with in infectious diseases, also frequently occurs in chronic diseases accompanied by great exhaustion of the system, such as phthisis, chronic diarrhœa, dysentery, etc., in which the normal processes of nutrition and assimilation are severely deranged, so that a part of the albumen, destined for the rejuvenation of the tissues, instead of being appropriated for this purpose, becomes converted into fat; the same occurs through the imperfect combustion of the non-nitrogenous substances in the lungs. Generally, in these diseases, the nutrition becomes deranged only gradually, and, if fatty infiltration of the liver takes place, the process advances, in most instances, at the same slow rate of the original disease. In yellow fever, however, the case presents a different feature. Through the deleterious influence of the infectious poison, the disintegration of albumen and the exchanges of matter take place quite rapidly, and the fatty infiltration of the liver may commence at a comparatively early period of the disease, and, in advancing, seriously interferes with the neighboring organs. For, besides the diminution and final suspension of the biliary secretion which it causes, it also interferes with the portal circulation by the pressure, which the abnormal amount of fat, deposited by the blood and absorbed by the hepatic cells, exerts upon the capillaries and inter-lobular vessels. And it is this pressure by which the congestion, representing the first stage of the pathological process in this organ, is gradually reduced. In advanced cases of fatty infiltration, therefore, the minute vessels are very rarely found over-filled with blood; on the contrary, the capillaries, as we have seen, are mostly found empty. The disturbance in the portal circulation, however, will soon be felt by the tributaries of the portal vein, for, while the passage of the

blood through the liver is rendered more difficult, a congestion of these veins will be the result, which, in extending backward to the minute venules of the mucous membrane of the stomach and intestines, will give rise to that peculiar congestion of these organs, already described. In the stomach, this congestion is mostly accompanied by an infiltration of hæmoglobin into the surrounding tissues; and, in severe cases, ruptures of the minute venules, or capillaries, situated between the epithelial and glandular layers, will occur, and give rise to that much dreaded clinical phenomenon, known as "black vomit."

The vomiting of black matters in yellow fever has always been regarded as a most unfavorable symptom. The morphological elements of the blood which these matters contain are derived, as we have seen, from the minute hæmorrhages caused by the rupture of the vessels just mentioned. It is quite obvious that such a hæmorrhage must result in the direct relief of these minute vessels from their congested condition, and that the small quantity of blood, thus lost, is in itself inadequate to cause the fatal issue of the case, though *it most certainly indicates a very diseased condition of the liver, as well as a low state of the whole organism*; and it is, in this respect, that it becomes an important factor in the prognosis of the case. Besides this, although the primary congestion and irritation of the stomach is, during the first three days, rarely accompanied by much pain, when pressure is made upon the epigastrium,—it will finally, when aggravated by the advancing fatty infiltration of the liver in the manner described, give rise to a sensation of dull pain and fullness in the organ, rendering pressure insupportable to the patient. The condition of the liver, and with it that of the whole organism, therefore, may be accurately diagnosed by that of the stomach, and will notify the physician of the approaching danger. Very rarely black vomit occurs before the fourth, or even fifth day; in most cases, perhaps, it takes place still later, at a time when the pathological changes in the various organs have attained a high degree, and when the nervous energy of the organism is nearly wasted, producing an almost complete depression and exhaustion of the patient.

If, after the elimination of the poison from the system, indicated by the cessation of the febrile process, the pathological changes of the parenchyma of the liver have not advanced to a very great extent; that is, if the protoplasm of the hepatic cells itself, has not undergone *true fatty degeneration*, and, if sufficient energy is left in the nervous organs to supply the wants of the organism—*black vomit may occur without proving fatal*. For, after the removal of the disturbing cause, the poison, the condition of the blood will gradually attain again its normal standard, the tissues will again be properly nourished, and, in consequence, the deposition of fatty matters into the parenchyma of the liver will cease, and, finally, the fat in the hepatic cells be reabsorbed by the blood. In this manner, quite a number of patients recover from black vomit. In fact, in most cases in which black vomit occurs, the patient feels relieved after the ejection of these matters, and imagines that he will get well. In children, especially, the occurrence of black vomit is not as often followed by a fatal issue as in adults. This is owing to the exchanges of matter, especially in those organs pertaining to organic life, being more active than in adult life; while, on the other hand, the organs of animal life, particularly those of the nervous system, are in children more impressible and sensitive.

In many fatal cases of yellow fever, especially those in which the third stage is protracted, the pathological changes of the liver do not remain confined to a simple *fatty infiltration*, but advance to a *true fatty degeneration* of the protoplasm of the hepatic cells, as I have described in the section of pathological anatomy. These cases, of course, as may be expected, prove necessarily fatal.

The relationship existing between the stomach and the liver, also exists between the latter and the intestines, and it is therefore natural that the same pathological phenomena should be observed in these organs, though inferior in degree. Accordingly, in a number of cases, hæmorrhages, similar to black vomit, are observed to take place from the mucous membrane of the small intestines, which, mingling with the mucous matters, pass per anum, though, sometimes, the black matters from the stomach may also be voided in this direction.

The infiltration and degeneration of the liver, associated with congestion and minute hæmorrhages of the stomach—black vomit—are so constantly met with in every fatal case of yellow fever, that they may safely be regarded as a characteristic phenomenon of this disease. In cases in which the black matters are not ejected during life, they will, almost always, be found in the system after death.

In taking now a glance at the kidneys, the pathological changes of which have already been fully discussed in their appropriate place, we find that these changes have, as in those organs already discussed, been preceded by a hyperæmic condition primarily depending upon the same cause, that is, a partial paralysis, or paresis, of the vaso-motor nerves. One of the first phenomena, depending directly upon the relaxed condition of the minute vessels, especially the arterioles, is the albuminuria, making its appearance quite early, on the second or third day of the disease. It is now generally accepted that this phenomenon, very frequently accompanying even mere functional disorders of the kidneys, chiefly depends upon an increased pressure of the blood upon the inner surface of the walls of the minute vessels of the glomeruli, as has already been noted. In yellow fever, the conditions necessary to the production of albuminuria, are rendered especially favorable by the relaxed state of the smaller arteries, so that, soon after the commencement of the febrile process, when the heart's action is increased, albumen may make its appearance in the urine, without being accompanied by other products indicative of organic changes in the organ. The degenerative processes commence at a later period, during the second stage of the disease, when the increased disintegration of albumen and exchange of matter, entailing a complete derangement of the nutrition of the various organs, has reached a higher degree. As in the liver, a slight reaction, resulting in a temporary stimulation of the secretory functions and following the general nervous depression, also takes place most probably in the kidneys. This stimulation may be caused by the accumulation of urea, or its components, in the blood, calling upon the kidneys for elimination from the organism. And it seems to be not impossible that this same cause, together with the deranged nutrition of the

epithelial cells, lining the uriniferous tubules, may also give rise to that abnormal product of secretion of which the cylindrical infarctions are formed. The nature of this abnormal secretion, and the manner in which the different kinds of cylinders are formed, have already been so thoroughly discussed as to require no further remarks in this place.

It has already been mentioned that the degenerative changes in the parenchyma of the kidneys are not observed to have taken place in the same degree in all fatal cases of yellow fever, but, on the contrary, differ widely from each other. From this fact, it may be presumed, that in those cases in which these changes have been limited, death cannot be assigned to them, even if the urinary secretion should have been suspended for an abnormal length of time before this event took place. Even in those cases in which these changes have progressed to a considerable extent, there is a sufficient number of uriniferous tubules left open for the passage of the secreted urine. And if, in some cases, a total suppression of urine really takes place, it is hardly due to the obstruction caused by the infarctions, but much more probably by the general *atrophy* and *degeneration* of the epithelium. But, even if this were the case, the question whether the suppression of urine is the immediate cause of death, as is believed by a large number of physicians, remains still undecided.

The question whether or not the nervous symptoms accompanying certain organic changes in the parenchyma of the kidneys, and called "uræmic," really depend upon an accumulation of urea in the blood, has been agitated for a considerable number of years, and is as yet not definitely settled, though, in the course of this time several theories, in a modified form, have been advanced. Thus, while a number of physicians regard the accumulated urea in the blood as the immediate cause of uræmia, others accept the theory of *Frerichs*, according to which these nervous phenomena are owing to the carbonate of ammonia resulting from the decomposition of the urea in the blood; while others, again, attribute them to the presence of the extractive matters. *Traube*, even, rejected these theories altogether, and attributed the symptoms of uræmia, consisting in convulsions, coma, vertigo, etc., to œdema of the brain. Since these different theories

were advanced, a considerable number of experiments and observations have been made in relation to the subject, for the purpose of ascertaining the truth, and almost all the results obtained rather prove the fallacy of the uræmic theory. I shall cite the views of some of these investigators. Thus, *Chalvet** found during the uræmic attack a diminution of the urea in the urine and blood, rising again after the attack, but without attaining the normal quantity. Accordingly, he rejected the theory of Wilson and Frerichs, based upon a retention of urea in the blood, and rather regarded the retention of excremental matters in general as the cause of the uræmic phenomena. He, moreover, doubted that urea was formed in the tissues at all, but were rather a secretory product of the kidneys. *Budde*,† who made quite extensive observations on this subject, opposes the uræmic theory based upon the poisoning by urea, and particularly points to the fact, that a diminution of the secretion of urea before and during the uræmic attack does not constantly occur. He says that it is true that such a diminution takes place with a simultaneous decrease of diuresis, though the latter is in many cases rather abundant during the uræmic state. Four cases, which the author himself closely observed and described, showed an abundant diuresis, and three of them, besides, an excretion of urea in a comparatively large quantity, namely: 25.7, 27.6 and 22 grms. In 25 severe and fatal cases of uræmia, which he collected, there were five with abundant diuresis up to the commencement of the attack, in one of which the diuresis even rose to from 1200–2000 Ccm. during the attack. The conclusion drawn from this is, that, even if the excretion of urea is considerably diminished, or temporarily ceases altogether, uræmia must not necessarily follow; it rarely occurs in amyloid degeneration of the kidneys, neither in a cachexia, as leucæmia, notwithstanding the very scanty excretion of urine in these cases. Finally, uræmia does not necessarily follow upon long persisting anuria, accompanying certain diseases of the kidney and urinary passages. *Budde* observed himself a case of anuria lasting 40 hours, not followed

* *Virchow u Hirsch*—Jahresbericht f. d. Jahr, 1867, Vol. I, p. 336, and 1868, Vol. I, p. 226.

† *L. c.*—f. d. Jahr, 1874, Vol. I, p. 347.

by uræmia;* he furthermore reports a case of a woman, of 24 years, with highly albuminous and abundant urine—during several ca. 1700 Ccm., a few times even to 2300 Ccm.—and considerable œdema, in whom slight uræmic symptoms were observed, disappearing again with the decrease of the œdema. Two weeks previous to death, severe diarrhœa and vomiting, accompanied by scanty urine and disappearance of the œdema, occurred; and in the last 132 hours total anuria with no discharge per os or per anum, and without any uræmic symptoms. The autopsy revealed: chronic nephritis in the second stage, amyloid degeneration of the spleen, bronchiectasis and phthisis cavernosus pulmonalis. The author then shows the untenability of the theory, according to which uræmia is caused by a poisoning through the extractive matters, or through the carbonate of ammonia, and endeavors to prove the incorrectness of the assertion of *Jaccoud*, that toxic uræmia clinically differs from that depending upon anæmia and œdema of the brain, and that the former can be recognized by an œdema otherwise wanting, as he found in his 25 cases of fatal uræmia six with œdema and anæmia of the brain without œdema in other localities, and six others with the same condition of the brain, and inconsiderable œdema in other places. For this reason, Budde sanctions the theory of *Traube*, which assigns œdema and anæmia of the brain as the cause of uræmic phenomena. In order to show how frequent these changes in the brain are met with, he collected and tabled these 25 cases of fatal uræmia which he observed in the Communal Hospital of Copenhagen. In 18 of these cases, anæmia and œdema of the brain were found; in four others, accumulations of liquid in the ventricles and in the subarachnoid spaces of the brain were present; in two more there was anæmia alone, and in one only, the brain was normal. These statistics support the theory of Traube in a high degree.

More recent experiments, consisting in the injection of urea into the blood, have neither been followed by convulsions, but

* A remarkable case of suppression of urine, observed by my friends, Drs. D. W. Brickell and J. D. Bruns, occurred very recently in this city. It was a boy, nine years old, who suffered from congestive bilious remittent fever, with a complete suppression of urine—duly ascertained by the use of the catheter—for 67 hours previous to death. His intelligence was perfect up to the instant of death, and no trace of uræmia was observed.

rather by a rapid excretion of the urea, proving what has been said before.

From the observations, cited above, together with others that occurred in this city, we have reason to judge that in those rare cases of yellow fever, in which a total suppression of urine really takes place, we are not justified in assigning it as the true cause of the convulsions, occasionally observed before death. A suppression of urine, therefore, can only signify an extensive degeneration of the secretory structure of the kidneys, the epithelium, and an exhausted condition of the nervous system, similar to the black vomit, indicating the desperate condition of the liver. At the same time, it must be remembered, that the secretory function of the kidneys is really not totally suppressed in all cases in which suppression is reported, but that, if they were properly examined, many of them would turn out to be only cases of retention, while in others the urinary function would be found to be only temporarily suspended. Thus it has frequently occurred that in cases in which the physician had even based his judgment upon the results obtained from the use of the catheter when pronouncing suppression, the secretion of urine had only been suspended, and was resumed soon afterward.* In many cases, however, suppression of urine is pronounced without the catheter ever having been resorted to. The fact that urine is found after death in the bladder in, at least, half the cases of autopsies, corroborates this assertion. We may then reasonably suppose, that though total suppression of the urine may take place in exceptional cases, it is not the immediate cause of convulsions and death, but that, on the contrary, the latter are owing to the diseased condition of the brain.

In the preceding section of this treatise I have shown, that in the numerous fatal cases of yellow fever in which I examined the brain, not only the smaller blood-vessels of the pia mater but also those of the brain substance itself, were found congested with blood, and that, in almost all of these cases, the congestion extended throughout the whole organ, and particularly through-

* *Dr. J. Dickson Bruns* reported to me a case, which he observed during the epidemic of 1873, and in which the secretion of urine was thus suspended for 36 hours, at the expiration of which time it was fully resumed.

out the pons varolii and medulla oblongata, in which the vaso-motor, trophic and secretory nerve-centers are situated. The hyperæmia of the brain depends upon the same cause as that of other organs, that is, upon a depression of the vaso-motor nerves, followed by a relaxation of the arterial walls. Gradually commencing before the beginning of the fever, it manifests itself already in the headache and general nervous depression during the preliminary or initial stage of the disease, but increases with the appearance of the fever, when the action of the heart becomes more violent, and when the arteries, in consequence of the relaxed condition of their walls, are no more able to withstand the increased pressure of the blood. And as these vessels cannot recover their normal tonicity as long as the battle between the organism and the infectious poison is raging, the congestion of the brain becomes persistent, and increases the already existing disturbance of the nutrition of the nervous tissues of this organ. In consequence, a considerable amount of the nervous energy, which in the normal condition is destined to supply the various organs, is now lost by being converted into heat, a condition which will last until the original cause of these abnormal processes, the noxious poison, is eliminated from the organism. If this condition of things lasts only a limited time, no organic changes will take place in the nervous tissues, but, on the contrary, they will, with the subsidence of the fever and the accompanying exchanges of matter, regain their integrity, as happens in all cases of recovery from yellow fever, and also in most other cases of hyperæmia of the brain. In the more violent cases of yellow fever, however, in which the febrile process is prolonged and particularly stormy in character, the hyperæmia becomes persistent and manifests itself by violent delirium, convulsions, or other serious nervous phenomena, which have been regarded as depending upon the degenerated condition of the renal parenchyma, and the ensuing uræmia. That this is not the case, but that, according to the theory of Traube, they in reality depend upon an œdema, or any other diseased condition of the brain, deranging the normal functions of this organ, I have already sufficiently shown in the preceding pages, and can, moreover, corroborate by the œdema and anæmia of the brain

which I observed, two years ago, in a man who had been for a number of years an habitual opium-eater, and who died under severe convulsions, to which he had already been subject before his death. In a number of fatal cases of yellow fever, the brain, as I have shown, is found oedematous, which here, however, depends upon a hyperæmia, a condition, it is true, opposite to that which Traube assigns to the uræmic convulsions. This apparent contradiction, however, is easily explained by the fact that some of the accompanying phenomena of anæmia of the brain, such as convulsions, may likewise be produced by a hyperæmia of that organ. In both cases, the symptoms depend upon a disturbance of the normal nutrition of the organ, arising, in the one, from a diminution, and in the other from a derangement, or complete interruption, of this function.

The deranged nutrition of the nervous tissues, caused by a persisting hyperæmia, will also eventually give rise to degenerative changes of these tissues, or their minute blood vessels. Such changes may be observed, in a number of fatal cases, in the fatty degeneration of the minute blood vessels and of ganglion-cells of the sympathetic ganglia, and, even, to some extent, in those of the cortex cerebri. The degenerations which I observed in the semi-lunar and first thoracic ganglia, consisting in the disappearance of the nuclei of the ganglion cells, and in the fatty and atrophied appearance which the latter themselves presented, cannot but exert a most deleterious influence upon the organs which they supply, by greatly diminishing the nervous energy which they need for the preservation of their integrity. With the degeneration of the nuclei of the ganglion-cells, which I regard as special reservoirs, or stores, of potential energy, the influence which these nerve centers exert upon the nutrition of their respective organs, must become considerably diminished. The pathological changes in the kidneys, supra-renal bodies, and liver, therefore, may stand in a close relationship with those observed in the semi-lunar ganglion, and other ganglia of the solar plexus. In the same manner may the changes in the muscular tissue of the heart depend upon those in the ganglion stellatum. But, judging from these changes, found in those sympathetic ganglia to which my examinations extended, viz.,

the ganglion stellatum, ganglion semilunare, and some others of the solar plexus, we may well presume that they had equally taken place in a number of other sympathetic ganglia, especially those of the cardiac plexus; and if, furthermore considering that the nerve centers which regulate the action of the heart, as well as its nutrition, send their stimuli through the ganglion stellatum and cardiac plexus, it becomes obvious that these changes cannot but exert a depressive influence upon the action and nutrition of this organ.

Thus it happens that, while during the febrile stage of the disease the heart's action is accelerated by the irritative influence of the poison upon these centers and ganglia, it is depressed during the second, but particularly the third stage, when the nervous energy becomes abnormally diminished by these pathological changes and the ensuing exhaustion of the nerve centers. And if this nervous depression passes beyond certain limits, a collapse of the organism must evidently be the result; but it may also terminate in paralysis of the heart, causing the death of the patient. In very severe cases, the nervous depression is frequently associated with a degeneration of the muscular tissue of the heart, which is likewise cutting off every chance for recovery. Excessive nervous depression, or paresis of the heart is, therefore, one of the most dangerous symptoms in the whole course of the disease, especially when it appears in the last stage of the disease, as it may then be associated with the degeneration of the muscular element, and, if not soon relieved by stimulation, must evidently lead to collapse. The feebleness and rapidity of the pulse, together with the diminution of the peripheral heat, especially of the extremities, indicate the approaching danger, the action of the organ being too low to properly circulate the blood through the peripheral vessels. The muddy, cyanosed appearance of the face, shortly before death, also depends upon this cause.

In taking a final review of the nervous phenomena of yellow fever, we find then that, while in the commencement of the disease they depend upon a paresis of the vaso-motor nerves, this condition is soon followed by an irritation of the different nerve centers mentioned, situated in the medulla oblongata, and giving

rise to the temporary acceleration of the heart and the irregular contractions of the cutaneous blood vessels during the hot stage; but that, in the second and third stage, this abnormal excitation is relieved by a general depression and exhaustion of the nervous system, in many cases associated with paresis, or even paralysis of the heart. But though the latter condition, which mostly takes place in protracted cases, may be the immediate cause of death, the hyperæmia of the brain is not relieved, but persists in a more chronic form until the fatal issue, when its presence will be revealed by the autopsy. That in fatal cases, without regard to the immediate cause of death, the neuro-paralytic condition of the blood vessels persists until this event takes place, is corroborated by the larger cerebral arteries, even the basilar, being found filled with blood, which is not the case in other diseases in which death occurs from other causes and without hyperæmia of the brain, where the larger and smaller arteries contract during the agony, driving the blood through the capillaries into the veins.

The pathological changes, observed in the supra-renal bodies depend upon the same causes, and take place in the same manner as those in the kidney, liver, and other organs; but, for the reason that so little is known concerning the true function of these organs, I shall forbear making any remarks as to the part they may play in yellow fever.

In comparing the clinical phenomena and pathological changes of yellow fever with those of other infectious diseases, we find that they resemble each other in many points. Thus, in the typhoid diseases, such as typhus, typhoid, bilious typhoid, and relapsing fever especially, the same nervous disturbances, caused by the influence of the infectious poison and the ensuing derangement in the nutrition of the nervous tissues, are equally met with. The extravasations of hæmoglobin and the general tendency to capillary hæmorrhage, as well as other degenerative processes in the glandular organs are also observed in numerous cases of these diseases. But, notwithstanding the similarity existing in the above phenomena, there are some other important points, in which yellow fever essentially differs from these typhoid or other kindred diseases. The difference particularly concerns the so-

called "blood-making" organs, the spleen and the lymphatic glands, which in the above diseases are almost always more or less affected, while in yellow fever they remain perfectly normal. This circumstance also explains the fact, that in these infectious diseases the blood is frequently found to have lost its coagulability, that is, it has in the true sense of the word become contaminated, or prone to decomposition, which never, or, at least, *very rarely* takes place in yellow fever, though it is believed by a large number of physicians. In the latter, the infectious poison appears to exert its noxious influence especially upon the morphological constituents of the blood, the colored blood corpuscles, inducing them to part with their coloring matter, the agent in the processes of oxydation taking place in the organism; and the congestions occurring depend less upon a true contamination of the blood, than, as has been demonstrated, upon a paresis or paralysis of the vaso-motor nerves.

Another difference may be found in the duration of the febrile process, the extent of which seems to be typical, as regards its length of time, to each individual infectious disease.

Besides, many of the phenomena of yellow fever are also met with in those infectious diseases, known as "*exanthemata*," and in the different forms of miasmatic disease, so that only a few symptoms remain to be regarded as *pathognomonic* of this disease. This leads us to the question, How then, may yellow fever be distinguished from the above named diseases, and how may sporadic cases be correctly diagnosed during the absence of an epidemic?—questions quite difficult to answer. In the commencement of the disease, and, even, after the febrile process has started into action, it is almost impossible to make an infallible diagnosis, as a number of other infectious diseases commence in the same manner, accompanied by the same phenomena. In the diagnosis of yellow fever, therefore, *the whole complex of symptoms must be taken into consideration*. On the second day, the general observation concerning the relative state of the pulse and temperature, to which I have referred before, may be applied. This observation consists, as will be remembered, *in the falling of the pulse on the second day, with a simultaneous rise of the temperature of the body*. The presence of albumen in the urine on the

third day, also, may to a certain extent serve as a pathognomonic symptom. If the disease runs on to its third stage, and black vomit should appear, all doubts about the nature of the disease, of course, must vanish. In fatal cases, the autopsy must decide the question from the pathological changes met with in the different organs, for *fatty infiltration of the liver, associated with that peculiar congestion of the stomach, together with black vomit*, are pathognomonic phenomena of yellow fever.

As regards the prognosis of the disease, the reader may draw his own conclusions from what has been said in the preceding part of this treatise.

[TO BE CONTINUED.]

ARTICLE V.

EXTRA UTERINE FŒTATION. By H. ROZENCRANS, M.D.,
Elgin, Ill.

On the 28th day of July, 1880, I was called twenty miles into the country to see a sick woman, a Mrs. J., aged 28 years, the mother of two living children. I found this woman suffering severely, and was informed that for three nights she had not slept. Her sufferings were referred mostly to the lower part of her back. I administered to her tincture of opium, and bromide of potassium, a liberal dose; this caused her to sleep most of the night. Examination of this woman satisfied me that she was a subject for an operation; that it would not admit of delay, as she was then very much emaciated; could not retain food, could not rest night nor day, because of great suffering. I advised her to be put on an easy spring wagon, on a mattress, and carefully brought to town for treatment. This was done. I then made a thorough examination of the case, and diagnosticated it a case of extra-uterine fœtation, and from the history of the case to be of about one year duration.

As there was evidently a considerable quantity of fluid in the abdominal cavity, and to satisfy myself more fully of the correctness of my diagnosis, I resolved to introduce the trochar. and did

so, about one inch below the navel, on the median line, and drew off at least two gallons of a dark heavy fluid. I then made further examination and could distinctly feel a solid substance in the abdominal cavity and pronounced it a foetus. I wrote down my diagnosis, and sent it to a physician living in a neighboring town (Victoria), fifty miles away, inviting him and two others to come down and assist me in a operation for the removal of an extra uterine foetus by abdominal section. Dr. Sutherland and Dr. Blake, the former of Victoria and the latter of Cueso, finally came down and assisted me in the operation, on the 20th day of August, 1880, in Indianola. I, after getting this patient in readiness, administered to her chloroform, and immediately cut down through the abdominal walls on the median line extending from one inch below the umbilicus to near the pubic arch. After cutting through the walls of the abdomen, bringing to view the cyst, I had the patient turned over on her side and at once cut open the cyst, from which flowed freely a considerable quantity of the same character of fluid that I had removed a few days previously with the use of the trochar. I then at once thrust my hand into the sac and grappled the foetus and at once removed it, feet foremost, dead, and weighing eight pounds. I then tore away the placental mass, and proceeded to the removal of the sac. This I found a difficult thing to do because of its numerous attachments; portions of it I cut away, portions of it I tore away; a portion of the omentum was removed with it. The right ovary was removed, after ligaturing the pedicle with a cat-gut ligature. The wound was then closed, leaving an opening at its most dependent part, and a tube introduced for drainage, from whence for many days a copious discharge of putrid matter flowed. To obviate internal bleeding I, immediately after closing the wound, placed two bricks on the abdomen for pressure, leaving them there for half an hour and then removing them. During the operation I used the carbolic spray and diligently, during the after treatment, kept injecting daily into the wound, through the tube, a solution of five per cent. of carbolic acid. The weather was very warm, averaging 95°. I applied pounded ice constantly for the space of three weeks to the abdomen, and for that length of

time gave a quinine pill three times a day, of two grs. each. Ordered a milk diet. I drew off her urine twice a day for a week, after that she voided it herself. Her first evacuation from the bowels was on the fifth day. After the operation I never gave her any cathartic medicine. She never suffered any pain after the operation ; the temperature of her body remained quite normal, her pulse was usually one hundred in the morning and would rise to one hundred and twenty in the after part of the day, for about three weeks. I consider that the constant application of ice prevented peritonitis, and with the diligent use of carbolic acid prevented pyæmia or blood poisoning. I discharged my patient cured, on the 6th day of November, seventy-eight days after the operation. This operation was performed on the 20th day of August, 1880, in the Town of Indianola, State of Texas. I was one hour and a half in performing it. I am not aware that a similar operation was ever before performed in that State June, 1881.

ARTICLE VI.

THE COURSE OF THE BULLET IN THE PRESIDENT'S WOUND.

By EDMUND ANDREWS, A.M., M.D., Professor of Clinical Surgery in the Chicago Medical College.

Having recently conversed with Prof. Frank H. Hamilton and others, who have specially investigated President Garfield's wound, I think a few of the facts elicited may interest the profession.

The symptoms are such as make it a matter of doubt what the real internal injuries are. The would-be assassin shot him from behind, but the exact direction of the pistol at the moment of discharge is not clearly known. The attending surgeons agree that the bullet entered the back four inches to the right of the center of the spine, and traversed downward and forward through the mass of spinal muscles, and that it struck the right eleventh rib, and thence passed inside the bony walls of the thorax. The bul-

let was a conical one of the caliber $\frac{44}{100}$, and weighed 200 grains. The point was truncated, probably to make the cartridge short enough to be used in a short cylinder. This fact, and the additional one, that the charge of powder was light, gives increased probability that in striking the rib its course might be deflected.

The attending surgeons very properly judged that vigorous probing was unsafe. However, Surgeon-General Wales, of the Navy, introduced his little finger, and thought he felt the liver, and that the upper edge of the eleventh rib was notched or injured. Immediately after the shot the President felt pain in the distribution of the right great sciatic nerve, followed not long after by pain felt equally in the distribution of the great sciatics of both limbs. It is not quite easy to account for this fact. A probe is said to have been introduced three and a half inches by another attendant; but every one knows that probes in such cases run into intermuscular spaces with surprising facility, and give very imperfect information. In addition to the pain in the legs, the President felt a sense of heat and deranged sensation in the skin of the right inner part of the thigh, and right side of the scrotum.

The patient vomited repeatedly, but threw up no blood, nor was there any stain of blood in the sputa, alvine discharges, nor in the urine. There was some retention of urine, relieved by the catheter. There was some, but not excessive, shock, followed by a fair, moderate reaction.

The surgeons at Washington believed the liver to be wounded, and judged it probable that the bullet had passed through that organ, and lodged in the anterior wall of the abdomen, or in the groin, possibly wounding intestines on the way. As general peritonitis did not supervene, the fear respecting the intestines subsided, together with the anxiety about hæmorrhage. A spot in the right hypochondrium showed an ecchymosis and tenderness and was thought to be the possible location of the ball.

Now what was the nature of the President's wound? As to the liver having received the bullet, the evidence seems only that the location of the wound rendered it probable. Dr. Wales thought he felt it with his little finger, but liver tissue is not so characteristic to the touch as to secure him against mistake,

unless he explored with an energy which would be dangerous and unjustifiable. If the bullet notched the upper edge of the eleventh rib, and thence entered the liver, it must have passed across the lower edge of the posterior part of the right lung, wounding that organ, opening the pleura, and piercing the diaphragm. In such case Dr. Wales would have to pass his finger through the pleural cavity to reach the liver. If this were the course, the patient should have spit blood, and had an escape of air, followed by pleuritis, and at least some pneumonia, neither of which has occurred. If the bullet was deflected downward by the tissues which resisted it, and by striking the eleventh rib, so as to escape the pleura and the lung, such a course would be likely to carry it behind the liver and not into it. I understand Dr. Hamilton that Dr. Wales' examination with the finger gave him the impression that only the upper border of the rib was notched. If this were so, the glancing of the bullet would throw it more forward instead of downward, and the wounding of the lung and the liver would be sure.

As the lung certainly escaped, and as there is not a single clear proof of the liver being touched, it seems necessary to suppose that Dr. Wales, examining, as he doubtless did, briefly, was not able to get an accurate idea of the condition. A splinter of rib driven downward might give him the impression of the upper border of the bone. It is also conceivable that a hasty examination of a somewhat stout man like the President might cause him to mistake the twelfth rib for the eleventh. At any rate there has not been a single decisive symptom of a wound of the liver, so far as I have been able to learn, though plenty of reason to fear its possibility.

Prof. F. D. Weisse, of New York, after experiments and dissections upon the cadaver, authorized the following report in the *N. Y. Herald* :

TO THE MEDICAL PROFESSION AND THE PUBLIC: In view of the fact that I have been reported as having for several days past been engaged in making some investigations bearing upon the probable nature of the pistol shot wound inflicted upon the President, and because the published statements are disjointed and vague, I desire to make this public statement of my theory and observations relative to the case. On the second day after the shooting of the President it seemed possible that the wound did not

involve the peritoneum, digestive canal or right kidney, and if so the track and lodgment of the ball were yet unsolved. Studying carefully the official bulletins and the statement of the physicians in attendance it seemed to me that the weight of evidence pointed to an injury of the sacral plexus of nerves. I determined to investigate the subject, hoping to be able to contribute a possible clew to the unravelling of the case. I conceived the theory that by some fortuitous circumstance the ball had been deflected from its course by striking and fracturing the eleventh rib; that it had passed along the anterior surface of the transversalis muscle that forms the interior muscle wall of the outer half of the loin, passing between the transversalis muscle posteriorly and the kidney and ascending portion of the large intestine anteriorly; thence into the iliac fossa (which is the bed of the right groin), between the iliacus internus muscle posteriorly, and the cæcum or commencement of the large intestine anteriorly; thence continuing downward and inward through or behind the psoas magnus muscle to the right side of the pelvis (or lower cavity of abdomen), there inflicting an injury to one of the nerve trunks of the sacral plexus itself, where the ball is now probably lodged. This being the supposed track of the ball it would plow through a mass of fat which exists between the muscle walls behind and the abdominal organs above mentioned in front. This layer of fat in President Garfield (judging from his physique as pictured and described), is probably from an inch and a half to two inches thick. Two nerves cross the supposed track of the ball, lying upon the anterior surface of the transversalis muscle between it and the fat—namely, the ilio-hypogastric and ilio-inguinal branches of the lumbar plexus. These two nerves pass round each side between the muscle planes of the abdominal wall, reaching the groin, where they emerge at the side of the median line at the lowermost point of the belly, distributing nerve filaments to the skin of that region and to the skin of the scrotum, which areas of skin they endow with sensibility.

The symptoms which seemed to lead to this theory were, first, that the President fell on the receipt of the wound without losing consciousness; second, that he vomited almost immediately after falling; third, that upon being asked immediately after the receipt of the wound if he had any pain, he answered, "Yes, in my right leg and foot;" fourth, the occurrence of tingling and burning of the skin of the legs and feet together with the excruciating pains and cramps of the same; fifth, that upon the cessation of these pains he was left with a feeling of soreness of the muscles and exaggerated sensibility of the feet and legs. To test the theory of the deflection of the ball and its course I investigated the qualities of the weapon that inflicted the wound and made special dissections of the region involved. As regards the pistol used I hope to find some inherent defects in it which might tend to bear out the probability that the ball fired from such a pistol could be deflected; I had even thought that if it did prove a reliable weapon; that the particular cartridge fired might have been a poor one. I obtained a British bull-dog pistol and a box of forty-four caliber cartridges, and upon inquiry was informed that as a weapon it was defective in many particulars, both as to pistol and cartridge; first, the cartridge

contains eighteen grains of powder in a very shallow cup, the ball weighing 200 grains; second, that the ball is cut off at the end and not pointed; third, that the shortness of the barrel causes a loss of the effectiveness of the powder at the muzzle in firing; fourth, that as a weapon of precision in firing it is defective in the feeble grip afforded by the small and awkward shape of the handle; also, that the shortness of the barrel tends to elevate the muzzle at the time of the explosion of the cartridge. Upon testing the effectiveness of the weapon I found first upon firing at a one inch board ten feet off that the ball went through the board, but did not have force enough to penetrate a second board eight inches beyond and dropped to the ground; second, firing into the trunk of a suspended cadaver I found whenever the ball struck a flat bone it would lodge in the body; if it did not impinge upon a flat bone of the chest it would at times go straight through, but more often would lodge at an opposite point beneath the skin. Firing into the abdomen it would lodge in the body, owing, probably, to the mobility of the organ. The above observations would seem to show the possibility of a ready deflection in the ball.

The ball that entered the President's body is described as having entered four inches to the right of the median line of the back, fracturing the eleventh rib. The eleventh is the most movable of all the ribs, being free at its anterior end and not steadied, as is the twelfth rib, by a muscle attached to its lower edge. The conditions favorable to reflection by impingement upon and fracture of this rib are—first, the mobility of the rib makes it like a hickory twig fixed at one end only, and it swaying upon impact would expend a good deal of the force of the ball; second, the fracture of the rib would contribute still further to break the force of the ball; third, the external surface of the rib being convex tends to deflect a ball (of all bones of the body the rib probably deflects balls most often); fourth, if the eleventh rib is pushed inward from behind it rises anteriorly, and if a ball struck it, it would tend to turn the ball on its axis and deflect it downward and inward; fifth, the clothing of the President must also be considered as an element contributing to diminishing the penetrating power of the ball before it reached the rib, but I have no data with reference to the clothing. The above conditions at the point of entrance of the ball as impinging upon and fracturing the movable eleventh rib seem to have afforded contributive elements to facilitate deflection of the ball downward and forward.

I obtained a cadaver six feet high, but the body not quite as stout as that of the President, suspended it so that the feet rested on the floor, making it assume as near as possible the position in which the President stood when shot. A twelve inch trochar (a steel rod one-quarter inch in diameter, pointed at the end and fixed in a handle), was entered exactly at the point of the President's wound and made to pin all the tissues and organs together, so as to steady them during the progress of dissection. A careful dissection of the right loin was then made, layer by layer, from the skin to the internal organs, from an area extending from the tenth rib above to the crest of the ilium below, and from the median line flaps of the layers were turned off to the side. In the track of the wound the skin, sub-cutaneous tissue, the latissimus dorsi muscle plane and that of the serratus posticus inferior

muscle were removed, exposing the tenth, eleventh and twelfth ribs and their intervening muscles; also the exterior surface of the transversalis muscle for the external half of the loin and the erector spinæ muscle for the inner half of the loin. The external intercostal muscles were dissected out of the intercostal spaces and the intercostal vessels and nerves recognized, as also the costal layer of the pleura bridging the inner portion of the spaces to line the interior surfaces of the rib; in the external portion of the intercostal spaces; after the removal of the external intercostals, the posterior position of the internal intercostal plane of muscle was presented. The costal layer of the pleura was separated from the eleventh rib and one inch of the rib corresponding to the probable point of fracture was cut out with bone forceps. The pleural cavity was opened, and, the finger being inserted, found a pocket extending down to the twelfth rib, as the pleural attachment rises posteriorly toward the spine and anteriorly toward the eleventh rib. Three planes of tissue covering the convexity of the liver were now brought into view and were presented in the following order:—First, the diaphragmatic layer of the pleura; second, dissecting this off exposed the muscle structure of the diaphragm (the diaphragm being attached to the eleventh and twelfth ribs); third, the peritoneal lining of the abdominal surface of the diaphragm. Cutting through the peritoneum brought into view the thick lateral superior border of the right lobe of the liver. Removing the diaphragm a little lower down there was exposed the external and upper border of the kidney, there being no peritoneum intervening.

A large director was slipped in anterior to the tip of the twelfth rib in a direction downward on to the internal surface of the transversalis muscle. It passed readily downward and inward, following the course of the loin into the iliac fossa. The twelfth rib was then cut through and dissected out with the bone forceps, carrying away its diaphragmatic attachment; the transversalis muscle was sectioned along the director, down to the crest of the ilium. The ilio-hypogastric and ilio-inguinal nerves were dissected out and preserved *in situ*. The hand was inserted, crowding the fat behind the kidney, with the large intestine (the peritoneal sac being intact) forward; the fingers passed with great facility downward and inward into the iliac fossa until the palmar surface of their tips rested on the iliacus internus muscle, the nails against the external and inferior surface of the psoas magnus muscle (this particular point is situated at the back and internal bed of the groin.) The fingers, when crowded a little, passed to the surface of the anterior crural and obturator nerves of the lumbar plexus; crowding them still further the lumbo-sacral cord of the sacral plexus and even the sacral plexus itself could be felt. The peritoneum was now opened anterior to the external convex border of the kidney, exposing the external lateral border of the right lobe of the liver. The fat from the kidney was removed, and the organ was recognized as bedded in the fossa in the posterior and inferior surfaces of the right lobe of the liver. A ball to reach the external border of the right lobe of the liver would have to enter the cavity of the peritoneum and then penetrate the liver.

The dissection being now completed and the relation of the organ

involved in the region of the liver being rendered appreciable, a careful drawing, by Mr. Max Cohn, of the region was made, which, upon subsequent comparison, was found to accord exactly with the illustration of the region made by our standard authorities. The dissection of the region thus seemed to give additional possibility to the theory of the course of the ball and its present lodgment.

On the morning of Thursday, July 7, I called upon Professor Frank H. Hamilton, M.D., and stated to him my theory of the probable deflection and course of the ball, giving him my reasons therefor and my investigations and the data I had obtained relative to the bullet and the pistol and the results of my dissections of the injured region. I then asked the doctor to give me a detailed statement of all the facts in the case that he had learned at the time of his visit to Washington as one of the consulting surgeons. This he kindly gave me, and among other symptoms stated that the President had called attention to a peculiar sensitiveness of the skin of the right side of the scrotum. An injury to the ilio-hypogastric and ilio-inguinal nerves, which lie in the supposed track of the ball, according to the theory advanced would account for this peculiar sensation. This, indeed, seemed to be a most happy confirmation of the probable correctness of the theory. Encouraged by Professor Hamilton I repeated my dissections on July 7, and he made an appointment to be present at a demonstration of the same on the following day. On July 8 I obtained several cadavers resembling the physique of the President, and at two P. M., in the presence of Dr. Hamilton, Dr. George F. Shrady, editor of the *New York Medical Record*, and other professional gentlemen, I repeated the dissections, pistol firings, etc. I propose to continue these dissections, and carry them still further, in order, if possible, to determine the probabilities as to how eventually the ball will be dislodged and where it will most likely point, if, perchance, it is where I think it likely to be.

FANEUIL D. WEISSE,

*Professor of Practical and Surgical Anatomy in the Medical Department
of the University Medical College of New York.*

The Professor having telegraphed July 8, to the Union Metallic Cartridge Company, Bridgeport, Conn., for data, with reference to the 44 cartridge, received in answer the following telegram:

UNION METALLIC CARTRIDGE COMPANY, }
BRIDGEPORT, CONN., July 9, 1881. }

In reference to the 44 central fire cartridge, would say that the exact amount of powder contained in the cartridge is twenty grains; weight of bullet 200 grains. The crimping or turning in of the shell in the end makes the cartridge shoot stronger.

C. L. RICHMOND.

The *Herald* also contains an interview with Prof. Frank H. Hamilton, who has been at Washington in consultation on the President's case, and who expressed the opinion that there is no conclusive proof of a wound of the liver.

Dr. Weisse makes it highly probable that the bullet glanced downward behind the liver without touching it, but the further

course of it as marked out in his theory seems very improbable. That the shot should enter the abdomen through the eleventh rib, slide down to the pelvis, and curve across the iliac fossa without wounding the colon, kidney, ureter, anterior crural nerve, nor either of the iliac arteries, and then, turning abruptly backward into the hollow of the sacrum, wound neither the bladder nor rectum, but injure there the roots of both the right and left sciatic nerves, so as to cause the pain in both feet and legs, seems, though not absolutely impossible, very difficult to believe. The supposed lodgment in the hollow of the sacrum without wounding arteries or viscera, and the injuring the sacral nerves so as to cause pain without paralyzing motion is highly improbable, for the motor and sensory fibers there run mingled in the same trunks, and would be alike injured. It is more probable that when the eleventh rib was shattered the corresponding costal nerve received a violent jerk, which affected the adjacent cord mechanically where the sensory tract is sufficiently separate from the motor columns to receive an isolated injury. This would account for the sensory disturbance of the feet and legs better than a wound of the sacral plexus. If the liver has been really wounded it would not necessarily be fatal. The statistics of the late war, published by the War Department, may be condensed as follows :

Uncomplicated gunshot wounds of the liver—Cases, 59 ; deaths, 34 ; mortality 58 per cent.

Gunshot wounds of liver, complicated with other injuries—Cases, 114 ; deaths, 74 ; mortality, 66 per cent.

The army statistics have very little value for determining the President's risk, because the correct diagnosis was frequently impossible, and the projectile used in the regulation musket was much larger than the one fired by Guiteau, still they show that even if the liver is really wounded it will not necessarily be a fatal case. As above stated, I consider the present location of the projectile entirely undetermined, but that the probabilities are against it being either in the liver, or in the hollow of the sacrum.

No. 6, Sixteenth Street, Chicago.

ARTICLE VII.

CREMATION. By Dr. C. W. PURDY, of Chicago.

Death made its advent into our world through the agency of a dreadful crime, and ever since Cain thought to conceal the body of his victim with earth (the first burial) has the disposal of the dead been an anxious question with the living.

Many contradictory and superstitious customs of disposing of the dead were in vogue among ancient nations and barbarous tribes, but these are of little use to us as precedents. Herodotus, Cicero and Lucian inform us that some Asiatic nations feasted on the slain, and murdered the sick and aged; that others threw their dead to ferocious animals; that others cast them into seas, lakes and rivers; that the Scythians buried them in the snow. According to Spondanus, the "Syrscanians flung their dead to the dogs; some Indians left them to vultures; the Celts took from them the vertex of the cranium, which they set in gold for goblets." But as man became more civilized, the voice of religion, reason and policy called for the careful interment of the dead.

The Egyptians attached to public burial an idea of the greatest honor, as indeed did also the Greeks and Romans, and it was considered the highest recompense of virtue, and hence the desire of becoming worthy of funeral obsequies was universally cherished. It was a crime to disturb the repose of the dead in their last abode. A reverence for tombs thus became a part of religious worship, and to pay the last duties to the dead was held a sacred obligation.

Man, impelled by his passions, always oversteps the bounds of reason. The affectionate zeal of afflicted relatives invented at length the art of giving a kind of perpetuity to the inanimate form, and thus the early Egyptians embalmed the dead; using every art capable of preventing the action of the air upon the body to prevent corruption, and to secure it in such a manner, that it might be kept without danger to the living.

Self love gave much encouragement to this art. It was

believed that as long as the body remained entire, the soul hovered near it. Thus they were kept as a most precious family deposit, and a sacred civil pledge. The inevitable result followed of delaying Nature's processes. Contagious diseases broke out in the midst of the Egyptians and baffled every remedy ; and all the embalmed were obliged to be removed to a distance. The great number of dead, after the carnage of battles, obliged them to resort to burning. In time the old practice was completely changed ; and tombs and vases were only used for the ashes of the funeral pile.

Long wars, frequent transmigrations, ruin and rebuilding of cities in time overturned whole countries, and bones confided to dust for centuries were exposed and disturbed unavoidably. As this was a profanation nothing short of the most awful sacrilege, it resulted at length in the general determination to adopt burning of the dead. They even excluded the venerated ashes from within the walls of cities ; and deposited the urns in places consecrated to burial. The highways were for a long time bordered with tombs and slabs of marble covered with inscriptions.

“One consequence of stationing tombs upon the roads beyond the gates was that towns attacked were not as liable to slaughter and destruction ; the citizens would leave their walls for the defense of those sacred remains.”

We will find the elements of our own customs by a glance at the rites practiced by the Hebrews, Greeks and Romans.

Inhumation was practiced perhaps more universally by the Hebrew than any other race, and hence the origin of the Christian burial ; and yet this is the only Jewish custom retained by them. Abraham bought from the children of Heth the cave of Hebron and deposited there the corpse of Sarah. He himself was also buried there, as was Isaac, Rebekah and Leah. It is believed, as commentators declare, that the remains of all the illustrious patriarchs were assembled in the cave of Hebron with the bones of Abraham. Vaults dug in the hill of Zion, under the foundations of the temple, and in the royal gardens, were destined for the last abodes of the kings of Judah.

The course of time brought but slight changes in the practices of this people, although they underwent such eventful vicissitudes.

Although burial was the chief method of disposing of the dead, it was not the exclusive one. Among the great, especially kings, some secured the honor of embalming, and cremation was occasionally adopted for the same purpose (Jer. xxxiv. 5); probably this custom was of short duration and only peculiar to a few.

The bodies of Saul and Jonathan, however, were burnt to ashes by the people of Jabesh-Gilead in order to rescue them from the insults of the Philistines. We are also told that "a continual fire that consumed the carcasses and the filth of the city, burned always in the deep pit of Topheth in the valley of the children of Hinnom. (Isaiah xxx. 33.)

To the Greeks belong the credit of more uniform practice of burning the dead than any other nation of ancient times. They inclosed the urns which contained the ashes of their dead in private houses, within cities, and sometimes in their temples. Some traced back the origin of the custom to Hercules, who wished to carry to King Licinius the last remains of his son Argivus who perished in battle.

In the history of the Roman nation, nothing is perhaps more interesting than the endless variety of laws, rites and forms which they observed toward the dead. One half of the ruins or emblematical antiquities remaining, relates to this.

Numa Pompilius was buried on the Janiculine Hill; the succeeding kings were buried in the Campus Martius, a large plain along the Tiber. Ancient Rome permitted none of her citizens to be buried within her walls, save the vestal virgins. In course of later times, however, great heroes were allowed the same honor.

"During the many wars of the Republic and the incursions of the Barbarians, the impossibility of interring all human remains left exposed, necessitated their disposal by burning. Again, experience had taught them that they could not with safety bury their dead among themselves and their dwellings. To protect the remains of their venerated chiefs or companions in arms, the funeral pyre was adopted in imitation of the Greeks. The ashes were carefully collected by the friends of the deceased, enclosed in an urn and deposited in a monument, or in the catacombs, some of which still exist." (Pascalis.)

Thus we observe that three principal methods of disposing of the dead have been practiced : viz., inhumation, embalming, and incremation. The first originated with the first family of man, and has been more or less in practice to the present time. The second, embalming, if not originating with, at least was most extensively practiced by the Egyptians, and the perfection of this art attained by them has long since been lost to themselves and to the world.

Cremation of the dead was in more or less general practice among all the ancient nations of the world, but more largely so by the Greeks and Romans. Of the origin of this practice we are not reliably informed by historians. It is more than probable that it arose through a desire to preserve the dead from the passions and fury of the living, in those ages when man's barbarous passions impelled him to pursue his victims after death. Witness the fierce Achilles tying the body of Hector by the heels to his chariot, and driving around the city of Troy.

But the general adoption of cremation by the Egyptians, Greeks and Romans, was always the result of necessity for the protection of the living. About the end of the fourth Christian century its practice was discontinued, owing to the growing power of Christianity, whose doctrine of the resurrection, with its strong attachment to inhumation, brought a great influence to bear against it.

One of the most solid arguments in favor of cremation is from a sanitary standpoint ; and this is what interests us more especially as medical men.

From medical societies should spring the germs of sanitary excellence, and it is eminently proper therefore, that we should give careful consideration to matters looking to so important a sanitary reform.

To appreciate more fully the sanitary advantages of cremation, let us first look at the ghastly record of our present burial system.

"The history of vaults, grave-yards and cemeteries is one long tale of desecration, robbery and outrage ; coffins have been rifled from time immemorial for treasures ; there is scarcely a museum in the world which does not bear ample witness of this

practice of spoliation ; even professional grave-diggers have dug up the bodies of the high born for the purpose of finding rings and other treasures buried with them."

The London *Quarterly Review* (No. 42, p. 380), says " that many tons of human bones every year are sent from London to the north, where they are crushed in mills and used as manure."

It is impossible to preserve the dead by burial from the outrages of the living. Grave-diggers are often employed to remove bones ; as a rule they are not scrupulous and are often drunk.

"Where are the thousands who were buried in the heart of Paris, and slept there for centuries in the tombs of the Innocents? You may to-day gaze on their bones and comment over their skulls. By order of the Minister of Police they were all dug up in 1707 and carried off to the catacombs. The bones were cleaned and arranged in the galleries ; in one the arms, legs and thighs, intersected by rows of skulls ; the small bones thrown in heaps behind. 'They were bourn there, we are told, by priests and tapirs!'" (Hawes.)

A New York correspondent affirms "that a number of men were recently employed as grave-diggers in one of the old graveyards of that city. Bodies were offered for sale on the ground to a party of medical students, who shrank from the horrors they witnessed. One coffin was found to contain a heavy decomposed mass like spermaceti ; it was used to grease the axle-tree of a cart. Another coffin contained the body of a woman aged twenty, as the inscription announced. She had rested for 100 years—laid there with what tears, what tender regrets of husband, brother or mother ! but now her head was rudely seized and kicked like a foot-ball from one ruffian to another."

Previous to the passage of the Burial Acts, 1852-56, violation of graves in Scotland, England and Ireland was systematic. "In most crowded burial grounds there was a bone-house ; and, in many, a burning ground for coffin wood."

You paid for your grave ; your friends thought you would lie undisturbed. If you were in a vault, your coffin might still be there ; but after a certain number of years not your bones. Your bones would be in the bone-house, or sold for manure ; your coffin burnt. In many parts of London coffin-wood was

used habitually for fire-wood, and coffin furniture (second-hand plates, nails, etc.,) was a well-known article with marine store dealers." (See Walker, "Gatherings from Grave-yards.")

A surgeon visited the burial-ground in Portugal street, April 27, 1839. He found two graves open, several bones lying about; and heaps of coffin wood lying about, some quite fresh, awaiting removal for firewood.

A gentleman writes to the *London Times*, June 25, 1838, of this same grave-yard: "I was shocked to see two men employed in carrying baskets of human bones to the back of the ground, through a small gate. I have twelve of my nearest relatives consigned to the grave in that ground."

A writer in the *Weekly Dispatch*, Sept. 30, 1838, says of St. Giles church-yard: "It is full of coffins up to the surface. Coffins are broken up before they are decayed and bodies removed to the bone house before they are sufficiently decayed to make their removal decent. This bone house is a large round pit; into this had been slid from a wheel-barrow, the but partly decayed inmates of the smashed coffins. On the north side was a man digging a grave. He was quite drunk."

This over-crowding of cemeteries and burial-grounds is one of the most dangerous abuses to which the system of burial is subject, and this is bound to occur in every grave-yard situated in populous districts. It only remains a question of time.

St. Giles and St. Pancras, of which we have been speaking, are said to be literally saturated with corpses; yet long after such was the case, the hearses streamed in to deposit their ghastly freight of human merchandise.

Look at most large towns—once a rural village, to-day a crowded city with a pest-house in its center. Such in time is the history of most burying-grounds. Look at those hundreds of plague acres closed by Acts of Parliament in England, and think what overcrowding did there.

Nor need we go so far from home for examples in this matter; look at the overcrowded condition of the cemetery of Trinity Church, New York, and on the malignant fever induced by overcrowding there in 1822, for an account of which see *Commercial Advertiser*, Aug. 7, 1822. Part of this same cemetery is now

walled up with its contained bodies, and the traffic of Broadway resounds above the once consecrated ground.

It is only a question of time when our own beautiful Graceland, Calvary and Rose Hill will have been overcrowded, and perhaps swept away by the resistless march of our city's marvelous and unprecedented growth. Already the voice of mirth and the prattle of our children may be heard any day over the sod of what, only thirty years ago, was our principal cemetery. Only a few years ago, and each day the dark caravans might be seen wending their solemn way northward to deposit the dead. In the same spot now is our most frequented and popular of public parks.

Where are the bones of those laid to rest there by our early settlers, and the founders of our city's greatness? Some have been carted away for a short time farther north, while some—many indeed—still lie beneath those public walks and driveways, while the wheels of revelry, and the tread of pleasure seekers sounds constantly over their remains.

“The church-yard sentiment is a false one, because it is untrue to facts. You think of the body in its quiet, hermetically sealed tomb; but time knows no such seal. The protecting slab, the solemn walls about the consecrated ground, are all changed to-morrow. The church-yard sown with flowers, its groves of drooping willows, the quiet, peaceful dead,—it is all a fancy. The voice of history tells us that in the past there has been no such seclusion, and what is to change the course of events in the future. The world is the world of the living, and attempts to make it a dead-house will break down in the future as it has in the past.”

From the *New York Commercial Advertiser*, August 17, 1822, see the following: “It is not long indeed since we have seen, in this populous city, the mangled remains of the dead transported from one burying-ground to another. We have seen two church-yards opened, and deeply broken over an extensive surface, leaving exposed to our reluctant curiosity shattered limbs of corpses, and the decayed coffins, until they were closed by a range of brick vaults.

“These extend now a great way into the streets, literally under the pressure of a thousand horses, carts and carriages. An earthquake might in an instant throw up from under our feet the stings of death, the poison of human putrefaction, and create unexpectedly horrid scenes and cruel contrasts. It is well known that our large cities have, within a few years, extended much beyond their former suburbs; it has been necessary to build in many instances on what was formerly a burying-ground,—to expel, as it were, the dead for the accommodation of the living.”

The over-crowding of grave-yards has led to a system of what grave-diggers call “management”; this is, simply making room for more bodies, and in some of the cemeteries has been carried to a disgraceful extent.

“The vaults of Enon Chapel and Clement’s Lane are 59 ft. 3 in. by 28 ft. 8 in., allowing an average of 9 ft. per person. 200 would quite fill the whole area; fill that area six times, then the coffins lie in tiers six deep, and the whole space could not possibly contain more than 1200, and yet instead of 1200, between 10,000 and 12,000 were crowded into those vaults at various times! What became of them? They could not all rest there, thousands of them had to be managed.” (Hawes, p. 27 & 28.)

The burial-ground of St. Cuthbert is situated in the center of the city of Edinburgh. (We quote from the *Scotsman*, March, 1874.) In the last fifteen years, 10,800 bodies have been placed there. Graves were re-opened up to 1874, some every seven years, some every three years; coffins with no plates were usually broken open, the remains were often not ripe (decayed), the coffin wood was burned, the remains heaped back pell mell upon others, to make room for more. At last but one or two inches of earth could be placed between the coffins. When all this came out in court the sheriff’s address amounted to a defense of St. Cuthbert’s.”

The following is quoted as a notorious fact in the current prospectus of the Woking Necropolis Co. of London, 1874:

“In 1850 the Board of Health condemned the cemeteries of Highgate, Kensal Green, Norwood, Nunhead, and Brompton, and declared that they must be closed in the interests of public health. A central burying-ground was to be purchased at Abbey Wood, and Parliament was to buy up the old cemeteries. It bought

Brompton only, and closed it—no, used it, and is using it now. The government is accordingly using at high pressure and remunerative rates, a grave-yard condemned by its own board over a quarter of a century ago.”

The necessary limits of this paper will not permit our dwelling at greater length on this over-crowding system of burying the dead. Those who wish to investigate the extent to which it has been practiced, and the sickening results, we would refer to a work published by Mr. Walker, entitled “Gatherings from Graveyards.”

Mankind, until late years, have not devoted much consideration to the investigation of causes which might so far corrupt the atmosphere, as to create mortal diseases during marked periods of time; or why these plagues should appear in the vicinity of stagnant pools, shallow waters, receptacles of dead animals, or vegetable substances, in the neighborhood of slain after battle, in cities ravaged by famine; why columns of air have been rendered so noxious that the very birds that attempted to fly through them fell and perished; why the winds disseminating these aërial poisons, transported the breath of pestilence into rich, fertile, and thickly inhabited districts.

With the story of such terrible devastations the pages of history are filled. Putrid and malignant fevers, and periodical diseases often make their appearance in populous cities, without any apparent cause. May not this cause, known to us principally by its fatal results, be the practice of interment in the very midst of our dwellings? Epidemics which have laid waste whole cities have originated in the practice of burying in temples and churches. Diodorus, of Sicily, speaks of pestilences that were produced by the putrefaction of animal substances. Egypt is ravaged almost every year by malignant fevers, and from that country small-pox has spread over all the earth. The waters of the Nile leave with their slime an immense number of aquatic insects, which, as they corrupt, fill the air with pestilential miasmata. Lucan speaks of the ravages of an epidemic in the army of Pompey, near Durazzo; this was caused by the carcasses of horses, killed and left upon the field. Ammienus Marcellius informs us that the camp of Constantine the Great, was desolated

in consequence of the same imprudence. Frequently the corpses scattered over the field after battle have been the source and origin of disease and death on a large scale. The war of the Swedes in the seventeenth century, occasioned in like manner the great pestilence of Poland. Long and obstinate wars had the same effect in Austria, Spain, and in many other countries.

As we write at present, a plague is devastating Nedjeff, in Mesopotamia. "In this place is the grave of Ali, son-in-law of the prophet Mahomet. To this place his followers send the dead bodies of their friends and relatives, because they believe to be buried near this spot will assure their souls admission to Paradise. Caravan after caravan arrive there daily and deposit their ghastly freight for interment. The whole country about Nedjeff has become one vast grave-yard. The frequent floods occurring in the Euphrates, inundate all the land on both sides of the river. The light covering of earth is swept from the coffins, they fall to pieces, and thousands upon thousands of corpses are left rotting under the oriental sun, filling the air with their pestilential odors." Is it any wonder that a correspondent writes, May 2, 1881, that during the week just ended an average of 56 persons died daily out of a population of 6,000.

Both animal and vegetable substances, if subjected to heat and moisture at the temperature of 72° F. are converted in a little time into a variety of gases, carbonous, carbonic acid, carburetted, sulphuretted, phosphoretted, hydrogenous, etc., with azote, all of which are deleterious and deadly in their effects when inhaled into the system. It follows that carcasses can not be long exposed to heated air without becoming hurtful to persons in the vicinity of them. It has been argued, that provided a thick or heavy layer of earth be interposed between the dead material and the external air, no such gases can reach us to do any injury, and that they cannot remain long in their specific nature, but must soon be absorbed or decomposed and destroyed. This argument is not in keeping with the laws of philosophy, or practical facts. Layers of earth, even to seven feet in depth, can no more intercept the transmission of gas into the atmosphere than they can preclude the filtration of water. The power of the one is to descend and the other to ascend through a permeable medium. Water descends by its weight and affinity or cohesion. Gas

ascends by a double power ; first, by the impulse given to each column of it in the focus of fermentation, from which it is evolved with such a force and elasticity that it requires a thick covering of lead and wood to check it in vaults ; secondly, by atmospheric pressure, the action of which upon the surface of a grave 32 square feet, is equal to a weight of about 71,424 lbs. The only advantage in the depth of a grave is that of rendering septic formation more slow, on account of the slow diffusion of heat through the deep ground ; the want of this is replaced in proportion to plentiful rains. We do not deny the gravity of gases under consideration, and that they do not rise in the air to a great height ; but this is no protection against them : winds will not only raise but transport them to our dwellings. When such air, loaded with putrid emanations, remains stagnant, where it is little renewed, its effects become most deadly. Experience has over and over again shown that diseases of the worst kind, such as malignant, putrid, and exanthematous fevers are the fatal consequences. (See Dr. Felix Pascalis on Intra-mural burials.

“ On the 17th of August, 1744, a body was conveyed to a vault in the Parish of Notre Dame, of Montpellier, attended by a numerous procession. While lowering the corpse, a man first went down to support the coffin, and fell senseless ; another followed to assist him, and though drawn out in time, nearly perished ; the third had the courage to descend with a rope around his waist, and had he not been drawn up immediately would inevitably have died ; the fourth, a strong and vigorous man, trusting to a robust constitution, dared the danger, and died as soon as he had entered the vault ; the fifth came out once to recover strength and returned the second time, staggered from the ladder and fell dead.

“ Dr. Haguenot was commissioned to investigate the matter, and having had the vault re-opened, made the following experiments :

“ 1st. The cadaverous feter was so tenacious as to adhere a long time to any substance which was left a few moments in the vault.

“ 2nd. Lighted tapers, chips, paper and tarred ropes, when brought to the edge of the vault, were instantly extinguished, as completely as if dipped in water.

“ 3d. Small animals, dogs and cats, became instantly convulsed

and died in a few minutes, birds only lived a few seconds on breathing the impure atmosphere.

"4th. The vapor or gas was caught in glass vessels, and, after being kept for six weeks, afforded the same qualities and produced the same results."

The whole of these were witnessed and certified by Dr. Hague-not and a committee of the faculty of the University of Montpellier.

G. A. Walker, author of the book previously referred to, declares that "of all the grave-diggers he has spoken to, not one has wholly escaped the effects of poisoning; many had been overpowered by the gases on commencing to dig."

At Paris, in 1852, three men died from inhaling an escape of gas from coffins, and Mr. Fourcroy declared that all the grave-diggers he had examined showed signs of slow poisoning.

Mr. Chadwick affirms "that the sexton's vocation entailed a loss of one-third of the natural duration of life."

When the coffin of Francis I was opened, at the end of the last century, the dreadful vapors drove the men back, though he had lain there 250 years.

In 1845 the burial pits closed up at Louis during the "Black Death," in the fourteenth century, were opened, and the men employed were overpowered by the smell.

It is said that Marylebone grave-yard, closed for more than thirty years, smells, as does also St. John's Wood.

Dr. Selmi, of Mantua, has taken the pains to bottle the air of some cemeteries in calm weather. He finds it to contain an organic corpuscle, which he terms "septa-pneuma." This, administered to a pigeon, in solution, developed putrid fever and destroyed the bird on the third day. (See "Cremation of the Dead," by Dr. Pietra, Paris, p. 9.)

In our city the old burying-ground before referred to has been discontinued for public burials since 1864. Dr. Rauch, Secretary of State Board of Health, in a pamphlet, published in 1866, maintained that this cemetery contributed to the contamination of the water supply of the city.

During epidemics of yellow fever and cholera, the pernicious influence of intra-mural interments in this country has been most striking. The epidemic in New York in 1822 was considered to

be the result of the overcrowded condition of Trinity churchyard cemetery before referred to.

Several instances are cited by Dr. Rauch, one of which was the epidemic at New Orleans in 1853. In the fourth district the mortality rose to 452 per thousand, which was more than double that of any other district in the city. In this district existed three extensive cemeteries, in which were buried the previous year more than three thousand bodies. In other districts the proximity to cemeteries seemed to aggravate the disease. Another instance was observed by Dr. Rauch personally during the epidemic of cholera in Burlington, Iowa, in 1850. No deaths occurred in the neighborhood of the city cemetery until about twenty interments had been made there, and then cases began to occur, and *always* in the direction from the cemetery in which the wind blew. Another instance was the epidemic at Norfolk and Portsmouth in 1855, reported by Dr. Bryant. Here 45 per cent. of the population died. Nearly all interments were made in the city, where the water-level is only six feet below the surface. The average depth of graves was about four feet, and, in many of them, three bodies were placed, one upon the other. These cemeteries were considered by Dr. Bryant a fruitful source of the disease.

In 1843, Edwin Chadwick presented to the Home Department a "Report on the Results of Special Inquiry into the Practice of Interment in Towns in the Kingdom of Great Britain and Ireland. Chadwick deduces, from the sum of evidence upon the subject, the conclusion: "That inasmuch as there appear to be no cases in which the emanations from human remains in an advanced stage of decomposition are not of a deleterious nature, so there is no case in which the liability to danger should be incurred, either by interment or by entombment in vaults, which is the most dangerous amidst the dwellings of the living; it being established, as a general conclusion, in respect to the physical circumstances of interment, from which *no adequate grounds of exception have been established*, that all interments in towns, where bodies decompose, contribute to the mass of atmospheric impurity which is injurious to the public health."

ARTICLE VIII.

A SIMPLE YET NOVEL WAY TO USE A CATHETER. By EDWARD C. HUSE, M.D., Rockford, Illinois.

The soft rubber catheters of Jacques and Nélaton, are a wondrous improvement over the old-fashioned urethral "spike" style of instrument. Doubtless hundreds of lives have already been saved by them which would have been sacrificed, as formerly, by the old barbarity.

We are all familiar with the bundle of one-eyed "spike" catheters, of varying sizes, in the show-case of every druggist, which, in fact, probably always will be seen there. Most of them appear to have been "in stock" a hundred years.

A device for using the soft rubber instrument to advantage, in a narrowed urethra was brought forward by Tiemann & Co., some time since, acting upon the suggestion of a well known professor in New York. The very absence of rigidity in the soft instrument, excludes its use in prostatic enlargement, if complicated with stricture. The "spike" instrument is of course highly objectionable unless the wire stylet be withdrawn, when it becomes, virtually, no better than the very soft ones.

The new design for stylet which Tiemann & Co. make, to be used on occasion, is undoubtedly an excellent thing. But if one will use the "spike," and, when it reaches the bulbo-membranous region (where it is apt to stop), will simply *withdraw* the stylet for an inch or a inch and a half, he will find substantially the same instrument in his hands as the new device, and that the rigid shaft, coupled with the soft, persuasive extremity are thus in his hands in a moment.

A case in point will illustrate this. Some two months since, O. B. S., aged 22, with a deep seated and tight stricture of three years standing, came to me for treatment. After dilating carefully for several weeks with *bougies à boule*, till a No. 6 could be got through, I resolved to use the *one-sitting* treatment, and accordingly operated with the divulsor of Thompson. There had been a prostatic abscess for somewhat over a year, the result, he

told me, of small and sharp bougies, Nos. 3 and 4, which he had *used upon himself*, after "reading up" on the subject. Some eight hours after the operation I withdrew a No. 12 bougie, which was introduced immediately after the divulsor had done its work, and attempted, by means of a "velvet eyed" soft catheter, to empty the bladder. It did not pass. The catheter, No. 12, was worked along, down to the prostatic region, and there stopped.

An experience of several thousand times passing the rigid silver instrument has shown me, as it ought to every one, that no *force* in such a case is ever admissible, or for that matter in *any* case where a catheter is used. *En passant*, how often this vital, or rather, mortal, principle is ignored! What a hecatomb of human sacrifice its disregard has cost! The vertebrated instrument of Squire did no better. What was needed was a rigid shaft with a *coaxing* end. Taking a No. 12 "spike," I went down carefully to the obstruction, withdrew, for an inch or so, the stylet, thus leaving a *climbing* or *roving* distal end to the instrument, pressed gently upon the shaft, and in a few moments a warm jet of urine showed me that the prostatic Rubicon was passed.

It is not easy, always, for country practitioners to get "velvet-eyed" or soft catheters, with complicated stylets and more complicated instructions. Especially is this true in winter, where muddy roads, ten foot snow banks or —40° place an embargo on the fulfillment of one's intention. It is just at this season that prostatic troubles are worst. For fifteen cents, and the simplest possible manipulation, many an accident, usually fatal, could have been and can be now avoided.

It is not my purpose, in conclusion, to claim anything startling or especially ingenious, in this article. It is simply a new adaptation of an old means to the fulfillment of a very important purpose, and thus the avoidance of a sunken rock, in our surgical navigation on which the life of many a patient and the reputation of many a physician, have been irretrievably stranded.

June 12, 1881.

Society Reports.

ARTICLE IX.

LANSING, June 13, 1881.

MEMBERS OF THE MICHIGAN STATE BOARD OF HEALTH,

Gentlemen:—At a meeting of the New Orleans Medical and Surgical Association, held May 7, the following resolution was adopted :

“ *Resolved*, That the special attention of the National Board of Health be called to the unusual prevalence, at the present time, of typhus fever, scarlet fever, small-pox, and other contagious diseases in the Northern and in some of the Western States. We, therefore, urge the necessity of immediate action, the most rigid railroad inspection from such places, believing, as we do, that they may be considered *dangerously infected*.”

There have been reported from various portions of this State, cases of small-pox, so that now it may be considered to have been pretty well scattered over the State. So far, the local boards of health have succeeded in keeping the disease from spreading to an alarming extent. In this they have been aided by the fact that by action of this Board in years past, thousands of people in the State were vaccinated, and thousands more have been vaccinated this spring. But, although no general outbreak of the disease has occurred, yet, from the great numbers of immigrants pouring into and through this State, and the general prevalence of small-pox in foreign ports from whence these immigrants come, there is danger of such an outbreak, and it seems to me some action should be taken to prevent the further introduction of small-pox into the State. In order that there may be careful thought on this subject before action, I send you the copy

of the following letter from Stephen Smith, M.D., Committee of the National Board of Health on the Prevention of Small-Pox, and would respectfully suggest that you give this plan some thought, and be prepared to take some action at the regular meeting of the Board, July 12, 1881. The letter is as follows :

*“Dear Dr. Baker :—*The only plan by which small-pox, spread by emigrants, can be brought under effectual control, seems to me to be this, viz., the concerted action of municipal, State, and National sanitary authorities. The special functions of each might thus be stated: 1. The National Board might appoint Port Inspectors who should examine every immigrant, and give cards to each with name and date of inspection ; if the person is found protected, give him a white card ; if unprotected, vaccinate him and give him a red card with name and date of vaccination. 2. Appoint Inland Inspectors, to be located at the great centers where the emigrant trains pass from one State into another, as at Buffalo, Ogdensburg, Pittsburgh, Wheeling, Detroit, Cincinnati, Chicago, St. Paul, Kansas City, St. Louis. These inspectors could examine the cards, allow those holding white ones to pass, but re-examine those holding red ones, and if vaccination is not effectual, re-vaccinate. State authorities could inspect at such points as the emigrants are likely to be diverted from the train and locate in the State. 3. The municipal authorities would follow the emigrant to his home and secure general vaccination of the community.

“Would your State Board be interested in taking action in this matter, or, do you not suffer from immigrants? The National Board cannot properly act unless State and local boards call for aid. If there was combined action of the State Boards, the National Board could coöperate at ports and on inter-State lines, and the whole machinery of sanitary organization could work in harmony and effectually exterminate this pestilence. Emigrants arrive at New York at the rate of 6,000 and 7,000 daily—large numbers unvaccinated, and all have been exposed to small-pox in foreign parts. It seems to me that there is a grand opportunity for the sanitary authorities of the United States to combine, each working in its sphere, and give an example of their power to exterminate a plague which will increase for a year or more un-

less we make such an effort. I have detailed this plan to our Health Officer and Dr. Harris, and they approve. I think our State Board will ask the National Board to aid in preventing the introduction of small-pox.

(Signed)

“STEPHEN SMITH.”

Such an inspection service as detailed need not confine its work to small-pox, but the introduction of typhus fever and other contagious diseases could be guarded against.

I trust this important subject will receive due attention.

Very respectfully,

HENRY B. BAKER, *Secretary*.

ARTICLE X.

CHICAGO MEDICAL SOCIETY. Stated Meeting June 20, 1881.

Dr. E. Ingals, President in the Chair. Dr. L. H. Montgomery, Secretary.

A valuable paper on “Cremation,” read by Dr. Chas. W. Purdy, will be found in this number of the MEDICAL JOURNAL AND EXAMINER.

The discussion was opened by Dr. G. C. Paoli, who said, that probably the greatest opposition to the adoption of cremation would come from the Orthodox Church, although he failed to see any antagonistic principle mentioned in the Scriptures. Such opposition had generally been made to any progress of science. Hence he called on the newspapers to enlighten the public on the subject, and impress the people favorably, for it was evident that grave-yards were very harmful, and that cremation had become a necessity.

Dr. R. E. Starkweather thought that cremation was soon to be adopted. A prominent bishop of the Church of England had lately expressed an opinion that it was only a matter of time. It was not, however, so urgent in this country as in the crowded cities of Europe. Monuments raised to the memory of the dead, he thought, were not so commendable as the foundation of

charitable institutions and erection of buildings for that useful purpose.

Dr. C. T. Fenn thought the worship of the dead was so deeply rooted in the human heart that it was a question whether we could banish the idea that the dead is laid to sleep in the ground. He had no objection to have his body cremated, but he preferred to have those of his relatives and friends buried.

Dr. Valin said that the disposal of dead bodies was the mooted question. What shall we do with decomposing organic matter? He proposed to turn dead bodies into a source of revenue by selling them for dissection, and to soap manufacturers or fertilizing establishments.

Dr. E. Ingals did not approve of using the ashes of cremated bodies as fertilizers, but he knew of no better use to which old excavated bones could be put. A large quantity of such bones had come to the surface in the sand of old Lincoln Park grave-yard. He asked whether worms really infected dead bodies? He thought no living being could thrive in them. He referred to a pretty general fear among some people of being buried alive. Many people favored autopsies on that account. Many stories had been repeated of dead bodies having changed their position in the coffin; this was owing to the relaxation that followed rigor mortis, and should be explained to the laity. Besides a living being could not breathe in a common coffin, and could not live more than a few minutes. However, the body could not be considered dead before decomposition had commenced.

The decomposition by rotting or by cremating was the same, except that one process was much quicker. In order not to destroy criminal evidence some examination would probably be made. He did not believe there was any legal act preventing cremation. He disapproved of the extravagance generally displayed at funerals.

Dr. C. W. Purdy was convinced that the horror now manifested for incineration would be directed towards burying, as soon as the first had been witnessed a few times. He said worms did exist in corpses that decomposed with access to the air, but not in coffins underground. Yet serpents had been found in old coffins. He did not approve of the utilitarian theory. He professed a great respect for the dead, which he said should not be

diminished, since it was one of the noblest feelings of the human heart, and he proposed cremation as a much more respectful procedure than burying.

The Society adjourned to October next.

H. D. V.

THE POISONOUS EFFECT OF INTRA-VENOUS INJECTIONS OF PANCREATIC MICROZYMES.

In a late communication to the French Academy of Sciences, A. Béchamp showed that all the known properties of the pancreas were dependent on its microzymes. Some experimenters, who had obtained toxic effects from the ferment of the pancreatic juice, desired to experiment with these microzymes, and see if their introduction into the organism would be followed by different results from those obtained in experimenting with pancreatine itself. They repeated their experiments with the microzymes of the liver, to have a point of comparison. They reached the following conclusions, which Dr. M. Raynaud read before the Academy of Medicine, March 8: 1. The injection into the blood, of isolated pancreatic microzymes, capable of digesting albuminoids, causes an almost instantaneous death, if their proportion is Og. 0001 for every kilogram in weight of the animal experimented upon ($00_{10,000,000}$ of its weight). It is impossible as yet to furnish a satisfactory explanation of the process of death, the only lesions observed being a congestion of the digestive mucous membrane, which ended in an effusion of blood, whenever death was relatively delayed. 2. The injection into the blood, of putrefied microzymes, which have evolved into bacteria, and are devoid of their fermentative power, does not give rise to any result. Hence the following important corollaries: That death could not depend on embolism, which, beside, has never been found; that the bacteria replacing the pancreatic microzymes and the microzymes of fibrine, are absolutely harmless, and that a complete and radical change of function has been the result of the putrefaction experimentally induced in these micro-organisms. 3. The intra-venous injection with the microzymes of the liver is totally harmless, a fact which renders more wonderful the action of their congeners.

Foreign Correspondence.

ARTICLE XI.

VILLERS-SUR-MER,
NORMANDY, FRANCE, June, 1881. }

MESSRS. EDITORS:—In crossing the sea with a large party of his own patients, a physician naturally interests himself in the symptoms of, and the effect of treatment upon, that invariable element of an ocean trip, sea-sickness. Dr. Beard, of New York, has lately written a pamphlet upon this complaint, and his chief weapon against its inroads is the potassium bromide, with which he advises physicians to intoxicate their patients before they go on board the steamer. This bromidism is then to be kept up during the passage, and thus, thinks Dr. Beard, sea-sickness may be kept at bay. I regret that I cannot endorse this opinion and this advice. In the first place I can hardly believe that any physician of calm judgment would place delicate women under such overwhelming influence of bromide for several days before they go to sea and continue it during the voyage. Secondly, I do not believe such treatment would overcome or prevent sea-sickness. Given in proper doses, while at sea, the bromides in my hands effected nothing whatever. Nitrate of amyl is another remedy suggested by others as well as by Dr. Beard. I used it in strong doses. It not only did no good whatever, but its penetrating, to many, nauseating, odor disturbed the stomachs of those who otherwise, perhaps, would not have been so ill. Dr. Beard thinks the galvanic current an excellent remedy in sea-sickness, but says its use at sea is impracticable. By cutting small openings in the cover of a Hall's French battery for the passage of the wires and afterward suspending the box, I

found no difficulty whatever in passing the Faradic current through the stomachs of my patients, the positive pole being placed in the epigastric region, the negative over the base of the brain. In one case this procedure seemed to effect a temporary relief. In all others it failed to accomplish any good whatsoever. The citrate of coffeine is also recommended. Its effects are *nil*. In short, I believe there are only two remedies for sea-sickness, and they are rather palliative than curative, but they do lessen the duration of the attack. One of these is *fresh air* taken in a supine position on deck, the other is indomitable *pluck*. If patients cannot be kept perfectly warm on deck it is better that they keep their berths. Cold at sea is extremely exhaustive. In one of my patients it caused an attack of neuralgia which exposed the lady to serious danger. Food of simple character, such as biscuit, oatmeal porridge, baked potatoes, well salted, and a little black coffee without sugar, will gradually accustom the stomach to food of more solid character. But the rule should be that if such simple nourishment be ejected by the stomach, a fresh quantity should at once be taken. It is not necessary to say that lemons, oranges (and perhaps apples), are indispensable.

A small mustard plaster over the epigastric region is sometimes a comfort, and hot water at the feet is a delight. But the *medical* treatment of sea-sickness is a snare and a delusion. It not only does no good, but I believe rather increases than assuages that feeling of unspeakable disgust, which led Charles Dickens to say on the first day out, that he was afraid he should die, and on the second, that he was afraid he should not.

In several voyages across the Atlantic I have as yet failed to meet a ship's surgeon who had any faith in medical remedies against sea-sickness. These gentlemen are as heretical in this direction as the veriest old sea-dog before the mast. They try to sympathize with sea-sick passengers but are quite ready to confess their absolute ignorance of effectual remedies. I am inclined to believe, however, that the doctors on ocean steamers are not overwhelmed by ambition. For the most part they are genial, pleasant fellows, ready and willing to do one a kindness. They go back and forth between two ports, year after year, quite satisfied, apparently, with their lot and their salary of four or five

hundred dollars. They are generally unmarried. Living costs them nothing. They have a good table, and their principal outlay is for the clothes they wear. So far as I can learn the majority of them are contented, or at any rate feel that they cannot do better than respond to the limited calls upon their professional skill at sea, and therefore do not attempt to secure anything better. It is a pleasure to feel that they do not expect to be feed at the end of the voyage. With the exception of the doctor and the captain and perhaps the first mate, I suppose there is no soul of the service of a sea steamer who will not accept a "tip" of any dimension. This is the one annoying drawback to the pleasure of the voyage. One of our humorous paragraphists recently said: "A first-class way to commit suicide by starvation is to live in a hotel and not fee the waiter." This strictly applies to a sojourn of ten days on an English ocean steamer. And, remember, unless you give at least ten shillings to the steward who has done nothing for you beyond changing your plates at table, he will not only not thank you in the slightest degree, but will become your enemy. It will not do to starve, for you have a European trip in mind, so you give your steward the ten shillings, or a sovereign, and feel disgusted at the mental degradation of English and European menials of every description. You are allowed to breathe in England and on the continent but hardly without a fee.

On this side of the water the American physician rather dreads to purchase medicines. He quickly appreciates the greater elegance of the preparations of leading druggists at home. If he wishes to procure a bottle of quinine pills he finds a rough looking article, moderate in price it is true, but bearing no comparison with our sugar-coated or our beautiful gelatine-covered pill. The English pill is covered with *French chalk*, of all substances. I was glad to find in London, after a prolonged search, the pills of McKesson & Robins. But the indications were that they were not much in demand.

In France a pill is usually coated with silver foil and is quite satisfactory. The general manipulation of the French druggist is much superior to that of the English pharmacist, but an American physician abroad is disappointed by his failure to find

the many handsome and neat preparations of the alkaloids which are so abundant in the United States. The cost of all medicines, however, is much lower in Europe than in our own country. For example, I purchased one-grain quinine pills at the rate of seventy-five cents the hundred. In London an ounce of the potassium chlorate may be purchased for five cents.

While in the latter city last month, I had the pleasure of making the acquaintance of Mr. Howard Marsh, an orthopædic surgeon of great ability, and a gentleman whose charming courtesy I shall not soon forget. Aside from his cases in one of the large city hospitals he has a special hospital for hip-joint cases, supported by charity. Many of his methods of treatment were unique and original. I shall have more to say about them in a future letter. Mr. Marsh is a young man who holds a high place in the esteem of his London confrères, and I am sure will one day win a reputation which will extend beyond his own country.

Mr. William Adams, of London, made many warm friends among our surgeons during the International Medical Congress at Philadelphia, in 1876. During a call upon him he kindly showed me his plaster casts of the contracted finger cases which are beautifully shown in his recent work on this subject. In the billiard room of his house he has a case filled with these casts. They show the hands upon which he operated. Side by side are the deformed and the restored members. His success is very remarkable. His methods are so original and so simple as well, that I would earnestly advise those of your readers who are interested in the treatment of this deformity to purchase and study the Adams' book. It is published in reprint by Mr. Blakiston, of Philadelphia.

During a recent stay of some weeks at Ventnor in the Isle of Wight, I was strikingly reminded of San Remo, on the Mediterranean, a place which stands very high as a resort for consumptives. Ventnor has the same configuration, in a lesser degree, as San Remo. It is packed away at the base of high hills which protect it from northerly and northwesterly winds, and, therefore, possesses a climate which is milder than that of any other locality in England. English physicians send their chest cases to Vent-

nor for the winter. It offers many of the advantages of towns along the Mediterranean Sea, and is preferable to Nice and Cannes, because less subjected to sudden changes of temperature and to winds which blow across snow mountains.

It is a poem in the beauty of its setting; its rambling, elbow-jagging streets which wander hither and yon at their own sweet will; its charming villas, thatched cottages and lovely gardens. Verdure is so abundant that in a country walk one finds every porch, spire, post, tree, rock and fence buried beneath a deluge of ivy vine. And in the woods the very ground is a mass of ivy leaves. These vines cover and cling to every possible support. Picturesqueness is a word which describes the whole, from the stately hills and the bold cliffs on the shore, to the humble vine-clad cottage of the peasant. I am writing from a charming little town in Normandy. All about me are the same stone farm houses which are so common in Ventnor, with thatched roofs giving a holding place to thick moss, wild flowers and grasses. I am led to imagine, therefore, that the peasant's houses in the Isle of Wight were built after models left in the island by Normans and their descendants. In Ventnor is a noble charity in the shape of the Royal National Hospital for consumptives and diseases of the chest. Dr. Arthur Hill Hassam was its founder. It is supported by voluntary gift and by the small sum of \$2.50 per week, which each patient is obliged to pay. The feature which renders this institution unique, is the plan of treating all patients on the "separate system." Every patient has his own chamber. This, of course, is accompanied by the mental advantage of sparing patients the depressing effect of the ward system of treating consumption. A large percentage of patients become restored to health, and the majority improve under the regimen of the hospital. The institution consists of eight blocks of two houses each, standing side by side. In the sleeping story these houses are only one room deep. Each room, therefore, has the same amount of sunlight. They face to the south and look out over an immense garden toward the sea. Patients are kept strictly to certain rules, which comprise directions for the bath, the hours passed in the open air and the diet. The grounds comprise twenty acres. Vegetables and milk are supplied by the farm.

No incurable cases are admitted. One hundred patients is the limit of the number received. Two additional houses, however, will soon be erected. Verandahs run the whole length of the hospital on the lower and second story, those on the ground floor being covered. So that patients may take fresh air even during rainy days. If space allowed, I would gladly give you details of the admirable success of this institution. It is one of the lions of the place, and Great Britain as well as Ventnor is proud of it.

I will ask your readers to bear in mind that Ventnor, Isle of Wight, is a place to which they may safely send those lung and heart cases which bid fair to improve under suitable change of climate. The real benefit to be derived from such change of course consists in the ability of patients to live out of doors several hours daily. To send incurable cases away from the comforts and surroundings of home has always seemed to me a needless cruelty. The cost of living in Ventnor during the months from October to June is quite reasonable. * *

ARTICLE XII.

VIENNA, June 16.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—The famous operation, the excision of the cancerous pylorus, has scarcely realized the bright anticipations of its creator, perhaps because the cases hitherto operated upon have not been judiciously selected. Some nine stomach resections have been already reported, but one of the patients still surviving. Even the first, Billroth's own, operated January 29, has recently (about May 26), succumbed to a recurrence of the disease. Besides these there have been doubtless several cases *not* reported, as I know personally in one instance which occurred in Vienna. It is of course to be expected that patients will wait long, usually too long, before submitting themselves to an operation so formidable. Yet in the Continental hospitals, where patients place implicit trust in the surgeon, opportunities will doubtless occur for a fair and thorough test of the value of this daring but after all rational innovation. The one case which has not yet been terminated

fatally, was that of Billroth's assistant, Woelfler, performed in April last, in which, according to the operator's statement, the malignant disease was confined absolutely to the stomach, all other organs, including the lymphatic glands, being apparently unaffected. As may be imagined, this patient will be watched with anxious interest by all friends of the operation. Woelfler has recently published a monograph, with plates, describing the operation and modifications as performed by Billroth, published by Braumueller & Co., Vienna.

Another novelty, less startling, but perhaps more valuable, has been within the last few months introduced and pretty thoroughly tested in Vienna; the local application of iodoform to unhealthy, particularly tuberculous, granulations. Brought out by Mosetig, it was soon used quite extensively in Billroth's wards, and with the most satisfactory results, according to the report of B.'s assistant, Mikuliez, at the surgical congress in Berlin. All indolent, granulating surfaces, but particularly the fungous granulations so often observed after chronic synovitis with ulceration of cartilage, have been observed to assume rapidly a healthy appearance, and exhibit an unusually favorable course after a few daily applications of iodoform crystals, so that this procedure has taken a recognized place in the armament of the Vienna surgeon.

By no means the least important of recent Vienna promulgations, are the researches of Dr. Jos. Gruenfeld, with the endoscope, or rather urethroscope. Ever since Désormeaux's presentation of his complicated endoscope in Paris, 1867, this branch of investigation has received more or less fitful and transitory attention from various specialists. Nearly all have contented themselves with the construction of instruments involving abstruse optical principles, culminating in the recently constructed, very beautiful, elaborate and expensive, but (for the urethra) comparatively worthless electrical endoscope of Nitze in Dresden, made by Leiter, in Vienna. Meanwhile, Dr. Gruenfeld, with very simple instruments, was training the *eye*. Beginning in 1871, as assistant in Sigmund's extensive syphilis division of the Vienna General Hospital, he has steadily and perseveringly pursued the study, and has finally, during the last year or two, by

means not simply of various publications on the subject, but also (and this is far more satisfactory) through many admirable demonstrations in the Royal Society of Physicians, earned deserved attention and recognition, not simply for himself and his method, but also for the subject—*urethroscopy*. This recognition took tangible shape in the form of an invitation by Billroth to write a volume for the “System of German Surgery” edited by Billroth and Luecke of Strassburg, now appearing. The book appeared last year, is being already translated into Italian, and will soon appear in English. That the author has somewhat exalted notions as to the future of endoscopy, is not unnatural; but that he gives valuable information as to the investigation and treatment of pathological conditions in that small but troublesome field, the urethra, is acknowledged even by his fiercest opponents and critics. I was so fortunate as to enjoy for several months Dr. G.’s private instruction, and propose in future letters to give the JOURNAL readers the benefit of my experience.

Another interesting and perhaps practically valuable work is the investigation of the origin and formation of vesical calculi, by the young but already well-known specialist, Dr. Robt. Ultzmann, of Vienna. The work has not yet been published, but will be presented to the International Congress, in London, in August, illustrated by numerous micro-photographs of sections through vesical stones. This work has especial value from the fact that the rapidly increasing popularity of lithotripsy à la Bigelow, is fast removing the possibility of convenient work in this line of investigation.

THE next meeting of the American Dermatological Association will be held at Newport, R. I., on the 30th and 31st of August and the 1st of September next. Papers are expected from many of the members, including Drs. White and Wigglesworth, of Boston; Duhring and Atkinson, of Philadelphia; Heitzman and Sherwell, of New York; Atkinson, of Baltimore; Hardaway, of St. Louis, and others.

Domestic Correspondence.

ARTICLE XIII.

FALLS CITY, NEB., June 6, 1881.

EDITORS MEDICAL JOURNAL AND EXAMINER: My attention was first called to the term, gangliasthenia, by an article in the *Michigan Medical News*, under date of April 11, 1881, written by Dr. E. Halsey Wood, of Hersey, Michigan; and according to this gentleman, "gangliasthenia means loss or diminution of ganglionic nerve power." "The term does not denote the condition of the ganglia, but expresses the fact that they do not generate a normal amount of nerve force."

Previous to the appearance of Dr. Wood's article I had seen and treated several cases of a peculiar nervous trouble, which seemed to be affections of the sympathetic system of nerves, as indicated by the symptoms.

In meeting with these cases, I was puzzled to know what to call them, and did not call them by any name, except that vague term "nervous trouble," which satisfies the people, and we can go on and treat. But this did not satisfy me, and ought not to satisfy any physician.

Upon looking for something on the subject, in our text books, I could find nothing in the books to which I had access; so I treated them according to common sense, and fulfilled the indications which the symptoms pointed out. Upon seeing Dr. Wood's article, it gave me a name which I thought was a good one for the trouble or disease I had met with, and which I think, will be plainly seen by the history of the following case:

On February 9, 1881, was called out five miles from town, to see a Mr. H. Upon asking the man who came for me (who was his brother), what seemed to be the trouble, he said his

brother was suffering with chills at the present, but had been sick for several months, and did not know what was the matter. He had been treated during that time, first by a couple of physicians from a neighboring town, and last by an eclectic physician from this place; but the patient did not improve. Mr. H.'s trouble had been called kidney disease, liver trouble, chills and ague, heart disease, dyspepsia, and his friends thought he was dying of consumption. Upon reaching the house, I found the patient to be a young man about twenty-five years of age; an American; had been married about seven months; a farmer; had been very healthy until about three months previous. The first symptom he noticed was his becoming very tired upon slight exercise; his back ached, he was languid; he did not rest well at night; was troubled with constipation, and with frequent frontal headache; his food was not digested well, and sometimes gave pain; his appetite was changeable; he became nervous and weak and began to lose flesh; was troubled with palpitation of the heart, slight cough, frequent flushes or rush of blood to different parts of the body, with a sensation of warmth or heat; frequent chilly sensations; localized sweating, such as one limb, the body, the hands or the head and face; insomnia; frightful dreams; blurred or disordered vision, congested conjunctivæ, convulsive closure of the extremities, and frequent epistaxis. These symptoms, slight at first, continued and gradually increased in severity. When I saw him, February 9, he was confined to his bed, suffering with severe palpitation of the heart, one or two severe chills a day, without much fever after them, sometimes he would have quite high fever without any marked chill; he had pain over the heart and liver; headache; sleep very much disturbed, and so nervous that if a person should touch the bed-covers it would startle him, even if he was asleep; his back was very weak; his bowels very irregular, sometimes constipated and sometimes troubled with diarrhœa; at this time he had no localized sweating, but would sweat all over very profusely, from three to seven hours at a time; would wet the bed-clothes and sometimes through the mattress.

I examined his heart and lungs and found them healthy, except with slight bronchitis, his cough was sometimes dry and

sometimes with free expectoration. I examined his urine, and found his kidneys in a healthy state, but his urine was at times free and abundant, while at other times scanty.

I will now leave the readers of this article to draw their own conclusions as to the relation of the sympathetic system of nerves and the foregoing symptoms, and the propriety of calling this trouble "gangliasthenia."

As regards treatment. I first had him give up the use of strong tea, of which he drank three or four cups a day; also the use of tobacco, of which he used a great deal; had him take a tablespoonful each of Scott's emulsion of pure cod-liver oil and Trommer's extract of malt, three times a day, and bromide of ammonium and potassium, each seven grains, tr. ferri chlorid. eight drops, and tr. digitalis, six drops every three hours. Sulphate of atropia, 1-80 of a grain, to be taken when the profuse sweating began. When his bowels were costive or constipated to take a teaspoonful of the following, night and morning:

R. Fl. ext. cascaræ sagradæ.
Tr. aloes et myrrh.
Syr. tolu, aa ʒj.

M.

Saw the patient again on the 15th, and found him much better; he felt stronger, was not so nervous, the chills were not so frequent nor so severe; his heart did not palpitate as much; his head ached very little; did not sweat so much; rested a little better at night, and did not dream so much; his nose did not bleed as often; appetite still poor; vision blurred; congested conjunctiva; still convulsive closure of the eye-lids; coldness of extremities and flushes of heat less frequent; cough no better. Continued the treatment with the addition of ten grains of chloral hydrate at bed-time.

March 2, was called to see Mr. H.; the family thought he was dying. I found him very much excited, respirations forty-eight in a minute, with considerable pain in the chest, skin dry and extremities cold. Gave him some brandy, with about twenty drops of fl. ext. of jaborandi, and had his feet bathed in hot mustard water. In about fifteen or twenty minutes he began to perspire quite freely; his respirations soon came down to twenty-four per minute. For two or three days after, cough was worse

and the sputa were rust colored, and the first day streaked with blood, indicating more or less congestion of the lungs. Continued the same treatment with an increase of ammonium bromide to ten grains. I did not see him again until March 24, at which time his cough had left; he could sit up all day; his appetite was better but digestion painful; his bowels were regular; profuse sweating had ceased; still had chilliness, but no marked chills; rested well at night, had no bad dreams nor headache; had no convulsive closure of the eyelids, vision was good; the hot flushes were very slight; had no palpitation of the heart; was still nervous and weak. Continued the oil and malt, gave citrate of quinine and iron ten grains, bromide of ammonium ten grains, and potassium bromide five grains three times a day, ten grains of lactopeptine after each meal, and cold salt water baths every other day, followed by rubbing with a coarse towel. This treatment was continued until April 5, when he was feeling about well, was able to be out of doors, but was still feeling weak. I withdrew all medicine, except the oil and citrate of quinine and iron, which he took twice a day, until April 15, when all medication was stopped, and to-day he feels better than he did before he became sick, but not as strong.

A. B. NEWKIRK, JR., M.D.

ARTICLE XIV.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—On April 17 I delivered Mrs. Rebecca Denning of an eleven-pound boy—full time. Mrs. D. is a native of Clay county, Indiana, where she was born the 13th day of January, 1827. She was fifty-four years three months and four days old at the time of her last delivery. She is the mother of thirteen children. She is a large woman, weighing about 200 pounds, of a sanguine, lymphatic temperament. Mr. John Denning, her husband, is sixty-four years past, hale and active, physically and mentally.

Yours cordially,

STREATOR, ILL., May 14, 1881.

JNO. J. TAYLOR.

ARTICLE XV.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—I was delighted to read the excellent paper of Dr. Clevenger in the May number of your splendid journal, on the labors of my old friend and classmate, Dr. Schmidt. Allow me to mention another piece of ingenious mechanism of his I saw utilized for a most excellent purpose, whilst I was taking my degree course in the Medical Department of University of Pennsylvania, whilst the Doctor was prosector for Dr. Leidy, which to me is entirely unique.

He took an articulated skeleton, and I am not sure but that he made that also, and on one side he sawed in two the bones of the extremities and fastened them together with a screw, and then made muscles of india rubber and attached them naturally, so by un-screwing it represented a fracture, and demonstrated the muscles causing the deformity; on the other side of the same skeleton were like muscles to show the deformity of luxations; this was for Dr. H. H. Smith, Professor of Surgery, and on me at least, they made a lasting impression, as I have never forgotten them.

The last time I had the pleasure of seeing the Doctor was in Selma, just after the surrender. I too had been Assistant Surgeon in the Confederate army, and like the Doctor, reduced from affluence to poverty. I was living in Alabama, my native State, not long afterward removing to this State, and I had lost sight of the Doctor, until reading Dr. Clevenger's paper, before referred to. All honor and a long life to the Doctor, for his devotion to our noble profession.

R. FOWLER, M.D.

AURORA, TEXAS, May 21, 1881.

PROFESSOR I. N. DANFORTH, of Chicago, has recently received from his Alma Mater, Dartmouth, the honorary degree of Master in Arts, a distinction which he has long deserved and which will in the future be succeeded by higher honors, if they who know him are not mistaken.

Reviews and Book Notices.

ARTICLE XVI.—SEWER GAS AND ITS DANGERS. By George Preston Brown. Published by Jansen, McClurg & Co., Chicago.

This little book is well worth reading, if for no other reason that it may impress upon the mind of the reader the extent to which the drainage of this city is defective. The author pursued his investigations in conjunction with the Chicago Public Health Department. His personal investigations have been very extensive, and therefore, although not a physician, his observations and many of his conclusions are of value. There is no way of correcting the existing evil of defective drainage, except by spreading a general knowledge of the chief faults in the present systems, and the best modes of correcting them. This is the object of the little volume under review. Fully one-half of the contents is taken up with descriptions of house drains which were allowing the escape of gas, that was plainly one, if not the chief, cause of the diseases which attacked the inhabitants. The faults pointed out, every reader will see, exist in a very large number of the Chicago houses. The conclusions which Mr. Brown arrives at in regard to the requirements of a perfect system it may be well to copy here.

1. There must be an unobstructed and positive ventilation of all the pipes and drains within and beneath the house.
2. The ordinary earthenware drains must be discarded, since they cannot be secured against breaking and defective joints.
3. Perfect joints and not mere connections must be made between sections of drains and pipes.
4. The waste-pipes and drains must be one continuous system, and made of material that will not break, leak, corrode nor

obstruct the free passage of the minutest particles which enter them.

5. Traps must be used that will not permit the escape of gas by defect in construction or use ; or, better still, they should be entirely discarded.

All should remember that the best disinfectant and the best insurance against disease is pure, fresh air.

RACHITIS AND CALCIUM PHOSPHATE. By Dr. Des Vallères.

The introduction of phosphate of lime in the materia medica is not recent. On perusing old pharmacopœia, we repeatedly meet formulæ containing burnt hartshorn, lobster's eyes, oyster shells, *album græcum*, etc., which consist almost entirely of calcium phosphate. Physiological experiments lately performed with that substance, by Drs. Chossat, Lassaigne, Cazalis, Guérin, Piorry, Gosselin, and many others, gave results which justified the reputation it held in the opinion of our predecessors.

The properties of calcium phosphate, brought to light by the learned investigators above referred to, may be thus summed up : *It has an immediate effect on the consolidation and the reparation of bones ; at the same time, and above all, it acts as an excitant of assimilation*—an important property, for what we digest and absorb, not what is ingested, nourishes the body. An idea, which could hardly fail to suggest itself, and it is a happy one, consists in associating peptine with calcium phosphate, after this has been rendered soluble and been changed into chlorhydrophosphate. I have been very much satisfied with it personally, and I have seen very good results follow its use, in puny children, lacking in energy, suffering from obstinate eye and ear troubles, from adenitis, very much predisposed to colds, etc. In a word, in all the sufferers from a scrofulous diathesis.

On the same principle, there is no doubt that phosphated peptone should be prescribed for pregnant women, who must furnish the foetus with the necessary phosphates, beside its other elements. It is a well-known fact that, during gestation, the

bones of the mother lose in their firmness; something, not so well known, is the report of Dr. Pégot-Ogier on that point. Here are the conclusions at which he arrived:

It results from the use of calcium phosphate that:

“1. In pregnant women, most of the common ailments pertaining to their condition disappear, and the number of stillbirths is diminished.

“2. The mother’s milk, often deficient in calcario-phosphatic elements, attains its maximum of richness which nature has fixed for the child.

“3. During infancy, youth, and until adult age, the development of the body progresses regularly; lymphatic nor rachitic diseases need be feared.

“4. The death rate, which, in Paris, is one to three (during the first year), has been brought down to one in five, which is the rate observed in the most salubrious places of the country.”

Poor and sickly nursing children are common indeed; this most often is owing to one of two causes; either the mother’s milk is too poor, or it is not well assimilated, the immediate result being an impaired digestion, which is often manifested by chronic diarrhoeas, with wasting, and a slow alteration of the economy, which may lead to rachitis. In that case, nothing is so rational as the administration of phosphated peptones to the nurse, and this will bring the milk to its normal standard, and furnish the child with the stimulant of nutrition, besides the elements necessary for the formation of the bones and teeth.

According to Liebeg and Humboldt, crude calcareous mixtures are administered to children to facilitate their *growth* and their *dentition*, in Germany.

Indeed, I might insist on the benefits which we can expect from that preparation in all diseases of the bones: caries, necroses, osteo-malacia, Pott’s disease, etc.; but I could not omit that to these diseases in particular does Trousseau’s recommendation apply: “Choose especially the time when the constituents of the child’s body are undergoing rapid changes, to renovate its constitution.”—*L’Union Médical*, March 1.

SOCIETY MEETINGS.

Chicago Medical Society—Mondays, Aug. 4-18.

West Chicago Medical Society—Mondays, Aug. 11-25.

Biological Society—Wednesday, Aug. 6.

CLINICS.

MONDAY.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Prof. Holmes; 3 p. m., Otological, by Prof. Jones.

Mercy Hospital—2 p. m., Surgical, by Prof. Andrews.

Woman's Medical College—2 p. m., Dermatological and Venereal, by Prof. Maynard; 3 p. m., Diseases of the Chest, Prof. Ingals.

TUESDAY.

Rush Medical College—3 p. m., Dermatological and Venereal, by Prof. Hyde.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Quine.

WEDNESDAY.

Chicago Medical College—2 p. m., Eye and Ear, by Prof. Jones.

Rush Medical College—2 p. m., Medical, by Dr. Bridge; 3 p. m., Ophthalmological and Otological, by Prof. Holmes; 3:30 to 4:30 p. m., Diseases of the Chest, by Dr. E. Fletcher Ingals.

THURSDAY.

Chicago Medical College—2 p. m., Gynæcological, by Prof. Jenks.

Rush Medical College—2 p. m., Diseases of Children, by Dr. Knox; 3 p. m., Diseases of the Nervous System, by Prof. Lyman.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Dr. Hotz.

Woman's Medical College—3 p. m., Surgical, by Prof. Owens.

FRIDAY.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Davis.

SATURDAY.

Rush Medical College—2 p. m., Surgical, by Prof. Gunn; 3 p. m., Orthopædic, by Prof. Owens.

Chicago Medical College—2 p. m., Surgical, by Prof. Isham; 3 p. m., Neurological, by Prof. Jewell.

Woman's Medical College—11 a. m., Ophthalmological, by Prof. Montgomery; 2 p. m., Gynæcological, by Prof. Fitch.

Daily Clinics, from 2 to 4 p. m., at the Central Free Dispensary, and at the South Side Dispensary.

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VOL. XLIII.—SEPTEMBER, 1881.—No. 3.

Clinical Lectures.

ARTICLE I.

CEREBRAL MENINGITIS. A Clinical Lecture by JAMES HUTCHINSON, M.D., of Maine.

This woman is 31 years of age, and was admitted on the 4th of February, 1879. Her father died of consumption, and one of her sisters of paralysis, but the rest of her family history, as far as it can be obtained, is good. Her mother says that she has been subject to convulsions since she was three years old, and that she began to show evidence of scrofula when quite a young girl. She has had frequent abscesses situated upon her extremities and trunk, from each of which dead bone has been discharged. Her mother also states that when she was 14 years old, and was living on a plantation in Virginia, she was attacked by her master on one occasion when he returned home much intoxicated, and was almost killed. She succeeded in making her escape from him, when it was discovered that her right shoulder was dislocated and that there was a subluxation of the left knee joint. She was so badly beaten and bruised that she was obliged to keep to her bed for a year afterwards. Her knees.

according to her mother's account, only began to swell after she was beaten. Since that time the convulsions, so-called, have been more frequent. Menstruation began at the age of 21, and has been very irregularly performed ever since. She has suffered much from dysmenorrhœa. The convulsions are much more frequent at the menstrual periods but are by no means confined to those times. In the winter of 1875-76, she had a succession of chills, followed by cough and expectoration, which was first tenacious and yellowish, but afterward muco-purulent and blood-stained. She had one or two slight hæmorrhages. She then suffered from headache, vertigo and faintness for about a week. Soon after this, while sitting at table, she fell to the floor unconscious, and remained so all day. She had no actual convulsion nor did she foam at the mouth. She was completely aphasic upon recovering consciousness. Her head was turned slightly to the left and rigidly held there. The left arm and leg were also rigid and powerless. The right hand was also slightly affected, and there was some rigidity of the right knee. There was also divergent strabismus of the right eye, with diplopia and flashes of light before the eyes. In one week the power of speech returned, and in the course of one month she had regained the power of motion in the right arm and leg and in the left arm. At this time the patient's bowels were constipated and there was retention of urine requiring catheterization. In the summer of 1876 she had anorexia, nausea and vomiting, and great pain in the abdomen which was very sensitive to pressure. She also had headache. About December, 1876, she had two copious hæmorrhages from the lungs. Soon afterward her articulation became imperfect, and in February, 1879, she had a violent convulsion which came on during sleep. On this occasion she foamed at the mouth and lacerated her tongue severely. She was unconscious for some hours, and upon recovering was again completely aphasic, and had paralysis of the muscles of deglutition, which lasted for one week and necessitated the use of nutritious enemata. There was no paralysis or rigidity of the muscles of the limbs or face, but her vision became much more indistinct than before. After this attack she was in bed about three months and during that time she was constipated and had occa-

sional retention of urine. The pain in her abdomen also returned after this attack. In August, 1877, she was admitted to this hospital, suffering from abscess in the side. While in the hospital she had a convulsion in which she foamed at the mouth and bit her tongue. After this she had aphasia for a short time. Three months later she had a fourth convulsion, and has been confined to her bed almost ever since. The abdominal pain returned after each of her last convulsions, and upon admission is still present. She denies all venereal taint, though she has suffered from sore throat. She has had a vaginal discharge for a long time. Upon her second admission the patient is quite thin and her body and legs are covered with cicatrices, the seats of former abscesses, but upon the left leg are several shining superficial scars. There are also signs of old synovitis of the right knee, and the ligaments of the left knee are very lax, allowing great freedom of motion of the leg in every direction. The middle finger of the left hand has its distal articulation semi-dislocated. There is divergent strabismus of the right eye. There is some dullness over the right chest posteriorly. The respiratory sounds are feeble and there is a trace of ægophony. The urine is acid; sp. gr., 1018; no albumen.

CLINICAL RECORD.

February 5. Ordered mist. gent. comp. f5ss. t. d. Patient had an attack last night, which lasted but a short time, in which she became unconscious without any convulsion.

February 10. Ordered potass. iod. gr. x. t. d. and mist. gent. comp. dropped.

February 12. There is a dwindling of the right leg below the knee. The right calf measures ten and three-fourths inches, and the left calf twelve and a half inches.

February 13. Patient complains of pain upon micturition. Vaginal examination reveals a prolapse of the womb and considerable leucorrhœa. Ordered injection of alum (gr. xxx to Oj of water).

February 17. Ophthalmoscopic examination reveals atrophy of the discs, the result of former choking.

February 18. Considerable pain in abdomen.

February 19. Patient says that she passes blood by stool. Once or twice during the past two weeks she seemed to have lost the power of articulation for an hour or two at a time (she has never lost consciousness at these periods), and when she has regained speech has stated that she had severe pain over the region of the heart.

February 20. Patient is rapidly improving. The strabismus is much less marked.

February 21. Ordered ol. morrhuæ, f3ss t. d. Iodide continued.

February 30. Convalescing rapidly.

POST-MORTEM EVIDENCES OF DISEASE IN CEREBRO-SPINAL MENINGITIS.—Rigor mortis marked. Posterior and lateral surfaces of body marked with livid spots of various sizes; also anterior surfaces of both arms and upper part of both forearms and knees exhibited little clusters of punctate spots, each of the size of the head of a small pin; some over the free border of the ribs on the right side. The peritoneum, and all the serous membranes exposed, presented a very deep, almost violet color. The pericardium contained a drachm of clear serum; membrane normal. The heart weighed three ounces and one-half; valves normal, right ventricle of auricle contained white clots; left ventricle contained fluid blood. The left pleural cavity contained some old adhesions at the base of the lung. The left lung weighed four and one-half ounces; hypostatic congestion of the lower lobe; crepitated throughout; diaphragmatic pleurisy. The right lung weighed five and one-half ounces, crepitated throughout. The liver weighed thirty ounces, was congested, and softer than normal. The right kidney weighed two ounces, the left two and one-half ounces, rather soft; cortex looked fatty, no irregularity in markings. The brain weighed two pounds and fourteen ounces; intensely congested, and in meshes of pia mater were collections of purulent material. The blood-vessels, after opening the dura mater of the cord, were found distended with blood, and in the meshes of the pia were collections of pus.—*N. Y. Med. Record.*

Original Communications.

ARTICLE II.

ADVANCEMENT OF THE RECTUS. By A. E. PRINCE, M.D.*

The operation of advancement of the rectus, from the crude state in which it was first proposed by Guérin (1849), has been improved and rendered more exact by many, among whom may be mentioned the names of Von Graefe, Critchett, Lebreich, Snellen, Knapp, Wecker, Pannus, Agnew, Noyes, Green and others, who have written upon the subject.

Yet the operation has hitherto remained imperfect on account of the difficulty of calculating the result in a given case, and the want of what may be called *adjustability*, rendering the operation incapable of being subsequently modified. It has hence always been regarded as a difficult and unsatisfactory operation rarely employed except in the most aggravated cases.

Though the limit of our space prevents a historic analysis of the progress of the operation, the introduction of Wecker's double-hook, one branch of which is designed to be inserted beneath the tendon, while the other slides down upon it from above, is worthy of special mention, for by thus securing the tendon until after the insertion of the sutures, the execution is freed from danger and greatly facilitated; though by his peculiar stitch, the tendency to split the muscle—or displace it upward or downward—or cause a slough of the strangulated area between the sutures and the cornea, is so great as to constitute insurmountable objections to the method.

The two pre-requisites of a typical advancement operation, are—
1st. Avoidance of the tendency on the part of the stitch to

* Reprinted from the *St. Louis Medical and Surgical Journal*.

divide the fasciculi and split the theca of the R. muscle, allowing it to retract, thus compromising or nullifying the effect, or even making the case worse. 2. A desideratum which has formerly received but little or no attention, is that of enabling the operator to *modify at will the effect of the operation after* recovery from the anæsthetic, and when the elastic tension and muscular contractility have returned.

The first of these indications is met by the loop-suture.

The second, by the *double loop suture* as employed in the following operation, which, while it seems to combine the advantages of other operations, avoids their objectionable features.

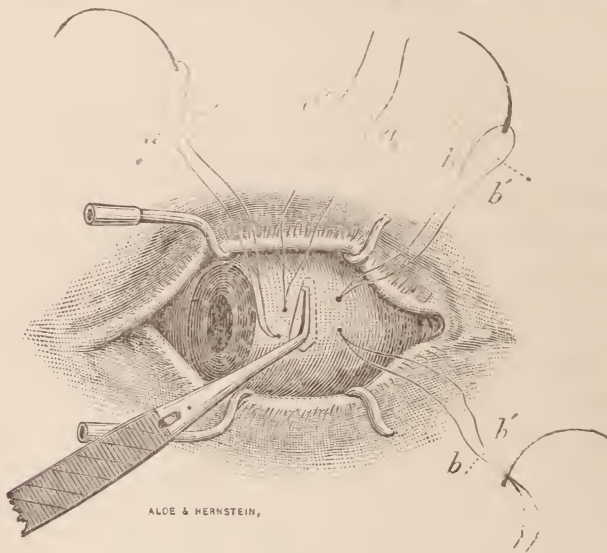


FIG. 1.

OPERATION.

The patient being asleep and speculum introduced, a fold of conjunctiva over the insertion of the tendon of the muscle to be advanced is grasped with the fixation forceps and elevated. A needle armed with a salicylized silk suture* is passed through

* The salicylized suture is prepared by immersing the silk on the spool in a solution of salicylic acid 1 part, alcohol 9 parts, glycerine 1 part; the end of the thread being allowed to protrude through a notch in the cork; thus it can be drawn out and cut off as wanted.

On evaporation of the alcohol, the glycerine retains the acid and keeps the suture damp and pliable. Such suture is no more irritating than the silver, until after the acid is dissolved, and is especially favorable when the cutting out of a suture is objectionable.

the elevated conjunctiva, parallel to and about two or three m.m. from the corneal-margin, after which the needles are cut off, making two loop sutures, as represented in the fig. (a) and (à).

A small opening through the conjunctiva and Tenon's capsule below and opposite the insertion of the tendon to be advanced is then made in the usual manner to admit of the introduction of one branch of Wecker's double hook or appropriate forceps,* which is passed underneath the tendon and drawn tense when the remaining branch is lowered upon the conjunctiva including tendon and cellular tissue.



FIG. 2.

This done, the tendon with conjunctiva is separated from the ball at its insertion, by the introduction of one blade of the scissors through the opening previously made.

Lifting the detached tendon from the ball by means of the forceps, the needles, carrying the *double-loop suture* are introduced from within outward through muscle and conjunctiva, as indicated (b) (b'); the position of the points of puncture depending on the effect desired. This step is facilitated by arranging the needle holder to carry both needles at once, which is important when no anæsthetic is employed, for the requisite time is thereby reduced to about two minutes.

Upon the introduction of the sutures the forceps is to be liberated by separating the contused tendon and conjunctiva, with scissors along the dotted line to the right in the figure, the needles being cut off as indicated.

A subconjunctival division of the opposite tendon having been made at the commencement when necessary, the advancement is accomplished by isolating and twisting sutures (a) and (b) which form secure loops, respectively through conjunctiva and muscle. The parts being cleared of blood, a knot is formed and drawn until the tendon is deemed sufficiently advanced, when it is secured by a simple bow. The conjunctival gap will have simultaneously been closed. After recovery from the anæsthetic,

* For forceps, address Aloe, Hernstein & Co., 300 N. Fourth street, St. Louis, Mo.

and upon the return of the muscular tonicity an examination is made to ascertain the success of the operation. The patient being directed to fix some object, there should be no motion upon alternately covering either eye.

If this be the case the remaining sutures (a') and (b'), after some hours should be withdrawn. Should the effect not have been sufficient, the sutures (a) and (b) are to be still farther tightened, as much as may be necessary to give parallelism to the eyes, and the knot secured, sometime after which the remaining sutures (a') and (b') are to be removed as before. Should, on the contrary, the effect of the operation have been too great, the stitch may be loosened or cut with the scissors and removed, the tendon allowed to retract, and the remaining reserve sutures (a') and (b') brought into requisition, twisted, formed into a knot, and sufficiently drawn to make the effect of the operation perfect.

The stitch may be allowed to remain until it becomes loose when it can be removed without pain.

The occasions for performing this operation, though quite numerous, may be naturally arranged in six classes.

1. Paretic and paralytic affections, where the muscular contractility cannot be calculated.

2. In case of over correction from a previous operation either from faulty operation, the muscle having been divided outside of the capsule, or in case of squint due to hypermetropia, the correction of which would have corrected the strabismus. When such eyes are straightened and the person subsequently uses glasses, the eye deviates in the opposite direction as much as it would originally have been corrected by glasses.

3. In case of strabismus of one eye exceeding 15° the muscular relations of the other eye being normal. In such cases one should confine the operation to the deviating eye, dividing one R. and advancing the opposite.

4. When both eyes deviate, amounting together to more than 30° after H. has been corrected. In such cases it is known that simple division of both R. Interni rarely suffices. A preparation for an advancement on the side of the greater deviation should be made, and the strabismus a little over corrected to meet the

cicatricial contraction of the two opposing muscles, thus accomplishing in one operation what is seldom a perfect success after several.

5. In case of a slight deviation, where we fear a simple division would produce an over correction. We should employ the advancement stitch to limit the effect of the division.

6. In case of binocular diplopia, this operation on account of its *adjustability*, is rendered especially applicable.

In order to put in practice the above, the reader must be familiar with the method of measuring strabismus by *degrees*, which alone seems the rational method of designating a deviation in the arc of a circle; and besides, the measurement in degrees, as will be conceded, is the easier and more accurate manner of determining the amount of deviation in any case of squint.

The observer should be provided with a perimeter or graduated semicircle, of from 15—18 in. radius; the degrees being numbered from the center toward the extremities and mounted upon a pedestal which will allow of various degrees of elevation, that it may be placed upon the same level with the eyes of different persons. The deviating eye is placed at the center of the diameter and the patient directed to look at the zero of the semicircle at which is placed a small colored disc, for a fixation object. If now a candle be placed immediately behind the zero point, and the observer's eye directly behind this; where no deviation exists, the image of the flame will be seen in the center of each pupil. Should the eye deviate the image will appear at some other point than the pupillary center. While the non-squinting eye continues to be fixed on zero, both candle and eye of the observer are to be simultaneously moved along the arc until the point is found, when the image of the candle flame is seen in the middle of the pupil. This point marks the corneal axis; and since the corneal and optical axes are approximately the same, this is the degree of deviation. When a still more accurate measurement is deemed desirable, it may be accomplished by ascertaining the angle A. It will frequently be found that when the optical axis is directed to zero, the corneal axis may deviate several degrees, which is found by moving the candle until the image is seen in the center of the pupil. The angle of inclination of these two axes, called

by common consent among oculists. angle A, is frequently large, causing persons to appear to have an external deviation, when really binocular vision exists. In such cases the optical axis cuts the cornea to the inner side of its zenith, when the angle is called positive, and should such persons have internal strabismus, the angle A of each eye should be added to, and in external, subtracted from the results of the former approximate measurements, which will usually be found sufficiently accurate. For this excellent and simple method we are indebted to Snellen of Utrecht, and Londolt of Paris,* in whose clinics alone the author has seen it employed; and it is hoped that the knowledge of such an admirable device will become more generally disseminated; for accurate measurements are essential to success, whether by the simple division operations or in cases where an advancement is indicated.

A few selected cases from our case-book will illustrate the above theoretical consideration, and serve to show the obstacles met with in some rare and difficult cases.

† No. 1016. Mr. A., aged 45, after correction of H. concomitant $S=35^\circ$, external motion quite limited. R. E. $V=\frac{20}{30}$, L. E. $\frac{20}{200}$. Seen through ground glass which prevents fixation, both eyes deviate about equally. Divided both internal tendons and introduced the advancement stitch for external R. of L. E.

The external muscles being quite weak found it necessary to advance to a considerable degree.

No. 1143. Master J., aged 12 years, concomitant S. 45° since infancy. No H., L. E. $V=\frac{20}{20}$, R. E., counts fingers at three feet. Division of both internal recti—and advancement of R. E. of the left. No difficulties.

No. 1329. Miss L., aged 18, concomitant alternating S. since 18 months of age, 50° . $H=1.5\text{ D }(+\frac{1}{24})$, after the correction of which $S=40^\circ$.

External motion of L. E. more limited than R. Divided both R. I. and advanced R. E. of the L. E. This not being sufficient, loosened the knot and advanced until perfect. No difficulty.

* Handbuch der Augenheilkunde. Graefe and Saemisch, Vol. III, p 233. The author employs a tangent to a circle in place of the regular perimeter.

† Above abbreviations. H=Hypermetropia; S=Strabismus; R. E.=right eye; L. E. = left eye; V=acuteness of vision; D=dioptric; R. E.=Rectus Externus; R. I.=Rectus Internus.

No. 1414. Miss P., aged 15, internal squint 50° $Hn=0$, $V=\frac{20}{30}$ for each eye.

External motion much limited in case of each eye, indicating a relative weak condition of both externi. Found it necessary to divide both interni and advance both externi. Cosmetic effect perfect, the latter R. E. being advanced by a second operation. Theoretically both R. E. should have been advanced at the first operation, but the necessity for the second operation is but testimony in favor of the rule—which, had it been followed, would have led to a perfect operation the first time.

No. 1370. Madame D., aged 42. When a child had both R. I. divided for internal S. In place of the tendon, the muscle was divided outside of the capsule of Tenon, and both retracted in the orbit. The external recti operating without any counterforce, an extreme external deviation was the result, amounting to 60° (35° in one eye and 25° in case of the other). It was deemed advisable to attempt an operation, though without much expectation of success. The external recti were accordingly divided, and to increase the liberty toward internal motion, the capsule was split meridianally above and below the muscles which were found to be exceedingly strong. No attachments were found internally, and no muscular fibers. Nothing except the most atrophied conjunctiva. Into this the double-loop suture was placed and the knot tightened to its limit. The temporary result was admirable. For a week there remained a slight internal strabismus which finally became converted into a slight deviation outward (15°), for which a second operation was not considered advisable. This case taught the important lesson exemplified in the next case, viz., that when it is a case of total paralysis, or due to complete separation of the muscle from the eye, the opposite muscle should be divided entirely outside of the capsule of Tenon, that it may be free to retract in the orbit, and exert no more influence on the eye than its paralyzed opponent. When such eye is straightened, we can of course expect only a limited amount of motion.

No. 1546. Mrs. B., *colored*, aged 26, congenital paralysis of third pair, implying loss of power over the superior, inferior and internal rectus, the inferior oblique and levator palpebræ superioris, which last was but partially affected.

The result of this is an external deviation of each eye of 50° , making a combined external strabismus of 100° with immobility of the eyes.

But a small portion of the pupil could be seen in the extreme external canthus of each eye, and when vision was attempted the head was held to the right or left, according to which eye was employed in vision, this being $\frac{30}{60}$ with each eye.

This being the most extreme case which had come to our notice, it gave us an excellent opportunity to test the merits of our operation, which worked according to programme, as will be shown by a photograph illustrating the condition, before and after the operation, and which will be mailed to any one upon receipt of stamp. The condition of partial ptosis remains to be corrected, the photographic views having been taken during artificial suspension of the lids.

From the above it will appear that the range of usefulness of an adequate advancement operation is by no means small, embracing some low and all the high degrees of strabismus, and especially such desperate cases as the above, and when we consider the small proportion of extreme cases heretofore attended by complete cosmetic success, and the large number requiring repeated operation, no apology will be necessary for bringing to the notice of the profession an operation whose *facility of execution, security and adjustability*, it is conceived, will assist in placing the correction of strabismus upon a basis approximating universal success.

ARTICLE III.

THE ETIOLOGY AND TREATMENT OF INEBRIETY. By Dr. CHAS. WARRINGTON EARLE, Physician to the Washingtonian Home, and Prof. Diseases of Childred, Women's Medical College, Chicago.

During the past ten years the writer has had unusual opportunities for investigating the causes of inebriety, as assigned by the men themselves. The results of treatment in the Washingtonian Home of this city have been noted, and the deficiencies, especially the want of a proper place for what I denominate the "third class," have been fully appreciated. Some of these reflec-

tions and conclusions, it appears to the writer, may be of some value to the medical profession.

In the first place, then, why do men commence the use of stimulants?

Second. After periods of reformation why do men again commence the use of alcoholics? and,

Third. How shall we treat them?

We are in medicine constantly speaking of the cause of certain phenomena, and, having found it, it comes to be an every-day expression in the treatment of patients, "Do away with the cause;" and so at the outset comes the question, "Why do men commence to drink stimulants?"

Answers based upon actual questions to many hundreds, and extensive observation, authorize me in saying, in regard to the

FIRST CAUSES OF DRINKING,

that association with drinking companions stands at the head of the list—forty per cent. giving this as the primary cause of deviation from a temperate life. In the majority of these cases the habit commenced in youth, and the particular stimulant used was *beer*, or whisky considerably diluted and flavored. It is the mild alcoholic at first, generally changed to the stronger as the appetite becomes accustomed and demands them.

Sociability comes next on the list, *ten* per cent. giving this as the reason. The custom of "treating," as it is called, is regarded as an evidence of sociability, and, in certain classes, is carried to a great extent. The first in a group of friends orders a particular alcoholic, and it is not strict sociability until all have imitated the example.

Trouble of various kinds, either in business or in the family, is assigned in ten per cent. of the cases. It will be observed, however, that after the habit is formed trouble of any character will, in a *greater* number of cases, be given as a cause of relapses.

The custom of drinking in families, so common a half century ago, seems to influence but few, and it is hoped that the time is not far distant when it will cease to be a cause. *Two per cent.* only acquire this habit in their homes. As a first cause

this habit cannot be too strongly condemned, and the people need education particularly on this subject. I am inclined to think that a much larger number owe the formation of an appetite to this cause than my figures seem to show. I frequently have to expostulate and condemn the practice, especially in those families where beer or wine is used to some extent. A few drops are given to the children. This I have noticed in some of the best foreign families whom I attend, and that it is regarded in the least dangerous to the future welfare of their children is quite a matter of surprise to the parents. As I remarked before, they need education in regard to it. Let them know the facts, and they will stop it.

Different kinds of business, which bring a person in contact with various kinds of alcoholics, has been a temptation but few can stand, and those engaged in the liquor traffic and in the hotel business sooner or later contract the habit, although a few, beginning to feel its effect in their own bodies, or noticing it in others, stop, and are temperate men.

Mental depression and active brain work are not frequent causes until after the habit is once formed.

Many young men, and a few of riper years, date the commencement of drinking habits to *army and navy associations*. During the war of the Rebellion many of the present generation were called from the restraining influences of home to share the vicissitudes of camp life and the excitement of the battle-field. It was at a time when they were forming habits for good or evil, and it can hardly be regarded with surprise that some contracted the habit of using alcoholic stimulants, which has followed them to this day. About *ten per cent.* give this as the first cause of their drinking. The habit is contracted at college by a few, and a great variety of excuses are given by others as the first cause of their drinking. A natural love for liquor is claimed by a few—exhaustion, poor health, sickness, etc., etc., are also cited as reasons.

I am thoroughly convinced that the great majority of boys and young men commence the use of alcoholics without the least idea that it is a dangerous practice. At first it is social; the preparations are agreeable to the taste, and the effect at this time, from

the fact that only a small amount is taken, is not perceptible. What the youth of our country need is education as regards the properties, and particularly the effects, of all kinds of alcoholics on the human system. It should be understood by every youth that the moderate use of mild alcoholics is not only exceedingly dangerous, but in a large number of cases, from causes from which I will presently enumerate, is absolutely ruinous. They should be taught to handle it as they handle aconite or arsenic.

I need not say here, for you will notice all through my paper that I believe that our only hope is in the education of the young. The great mass of those addicted to the intemperate use of liquor to-day will never be temperate citizens. Many of them will have spasmodic periods of teetotalism, only to succumb to the appetite, and for a time lapse into their former habits.

This brings me then, to speak of

RELAPSES.

or to consider the "Second Part" of my subject.

After periods of reformation, why do men relapse into former habits?

Unfortunately many who have apparently commenced a successful reformation begin to drink again. This unfortunate circumstance on the part of men who have stood up well for years causes, I think, more anxiety and discouragement than anything else connected with the treatment of inebriates.

It is not a difficult thing, after a period of intoxication, with the disgrace and physical infirmities conspicuously present to one's own self to induce the victim of this unfortunate habit to abstain from further drinking for perhaps several months, but many, as I remark above, again drink. To these men the question propounded has been, "What reason do you assign for drinking since you attempted reformation?" In the main I believe, as I do in regard to the reply given to my first question, that the answers are conscientiously and truthfully given. In the majority of cases it is a subject which has been seriously and carefully contemplated. The young man who is forming the habit of drinking, surrounded by gay companions, and taking the mild alcoholics, does not give the subject of reformation a thought; but

when a person begins to feel the power of the stimulant over him, and especially after he has lost his business and ruined his family, he reviews with great earnestness the causes of his relapse.

At the head of the list must be placed *trouble*. After the man has once formed the habit, the moment trouble comes, either in his family or in his business, or from grief at the loss of friends, or in disappointment at the loss of position, the unhappy man rushes for some variety of alcoholic stimulant. The man does this, too, fully realizing what it will do for him. It is the sheerest nonsense to say that he cannot help it. He does not want to help it. What he *does* want is to become oblivious to his trouble, and he knows that alcohol will produce this desired condition. Alcohol is one of the anæsthetics, and under its influence trouble, and sorrow, and disappointment are forgotten. *Sixty per cent.* of our relapses come fairly under this category.

Dissolute and wicked companions, either male or female, rank second in the causes of relapse. The young man just contracting the habit does not usually associate with men who are absolutely stained with crime, and he may continue for years to occupy his position in good society, and may enjoy to a considerable degree the confidence of his friends. Sooner or later, however, he commences to fall into bad company, and by the time his attention has been directed to any reformatory measures he has been discarded by many of his friends, and he has become the associate of the dissolute and outcast of the earth. A man of this character, who from any cause has had his attention directed toward reformation, always finds it a hard task to accomplish. Everybody, except his evil companions, has lost all confidence in him, and however well he may do, it is with great difficulty that his friends are induced to assist him in the least. They have long since ceased to give him more than a passing thought, and he is ostracised by all save his boon companions. It seems cruel that a man, under almost any circumstances, can procure a drink from a saloon-keeper, while the same individual, when trying to gain a foot-hold after the commencement of a reformation, *cannot get a cup of coffee*, and *hardly* the crumbs which fall from the rich man's table. I have frequently seen a man completely discouraged by the refusal of his friends to help him to merely a suit of

clothes, or to secure a subordinate position. Rejected by his friends, disappointed in every effort to obtain an honest livelihood, he knows of one class of people who will share all with him, and in his desperation he goes back to his old companions. *Twenty-five per cent.* relapsé in this manner.

Bodily suffering and chronic diseases. Many persons never enjoy perfect health. They suffer from a feeling of prostration. They are not robust and healthy. This may come from their parentage, or from subsequent illness. These people may have acquired the habit from some of the first causes I have enumerated. They have learned from experience that stimulants, for a short time at least seem to supply this want of nerve energy. It will bridge over, as it were, the feeling of prostration. The time comes, however, when this class of people desire to stop the use of alcoholics. They place themselves in an institution where, surrounded by temperance influences and supported by good diet and tonics, their reformation seems quite probable. An exposure, however, to causes which develop their physical weakness, produces in a number of cases a relapse. Men too, who are suffering from exhaustion and various painful diseases, have repeatedly told me that it was the cause of their relapse. A gentleman suffering from lupus of the face, of twenty years standing, assured me that it was the cause of his continued drinking. Patients who suffer from chronic lung disease frequently take alcoholics to quiet pain and relieve exhaustion.

At least fifteen per cent. fall from the causes just enumerated.

Night work, and the occupation known as "commercial traveling," cause many men to fall who had made every promise of success. Several years ago I made an estimate based on the admission of a thousand men into our institution who worked nights, and whose occupation deprived them of home influences and exposed them to an irregular and changeable method of living. *Fourteen per cent.* of the number I made the calculations upon, had relapsed from this cause. Recently I have carefully verified these figures, and find that almost without an exception the habit was formed before these men engaged in business, and this mode of life caused a *relapse*.

The absolute love for alcoholics, both as regards its taste and its pleasant effects when indulged in moderately, is a cause of a few relapses; but there is found in a majority of these cases some other facts, such as trouble, reverses, or evil companions. Certain customs, which are regarded by some of our most radical reformers as prolific causes of intemperance, I find exercise a very insignificant place among the reasons assigned for drinking at first, or as a cause of relapses. I allude to the prescribing of stimulants in combination with certain medicines by physicians; to the habit of offering wine to visitors on New Year's, and the partaking of wine at the communion-table. I have found two men who claim the first as the cause of relapse—one from taking wine on New Year's day, and have heard of those, but have never seen one, who dated his downfall from partaking of the sacrament.

I do not desire to have it understood that I favor the first custom, only when absolutely necessary, or that I advocate the second, somewhat ancient and fashionable habit of society, or commit myself as regards the propriety or impropriety of the use of ordinary wine in churches. My deductions are statistical. I can say this, however, that notwithstanding all that has been said and written against the use of alcohol as a medicine used properly and when needed to save life, there is no considerable number of physicians of any sect or school who feel that their entire duty is performed in the treatment of prostrating and septic diseases until the properties of alcohol as a stimulant and disinfectant have been tried. I was educated in a hospital where alcoholics were sparingly, almost beggarly, used, and I have stood by the bedside of the man with pneumonia and seen him die, whereas now I know, with ten years' observation, that a proper amount of this drug would, in all probability, have saved him. I have listened to the windy and stentorian arguments of the temperance lecturer and heard him declare that he would prefer—aye, a hundred times—prefer death to the prescribing of any alcoholics to him, even if on his death-bed. I have seen the same man and men on their sick-bed, not dangerously ill, but comparatively so, and long before there was any demand for it, have been told to exercise my own judgment in regard to the matter of stimulation.

In regard to communion wine being a cause of intemperance, I desire to say that particular stress and importance is paid to this by some at least who have no sympathy whatever with the church. If a single person has made the statement that he was tempted by partaking the elements commemorating the death and crucifixion of our Saviour, the statement has been exaggerated and frequently misrepresented by men guilty of profanity and licentiousness, and who never for a moment stop to consider how many hundreds have been saved by the influence of these organizations.

This may be somewhat foreign to my subject; but presently in this paper will come the question, "Who is to wage the fight against intemperance?" Is the attacking party to be made up of a few temperance societies, who are constantly berating the churches for fostering and aiding, and tempting men to become drunkards? Not by any means. Every temperance man and woman, whether they can subscribe to all the ultra ideas of the most stalwart, should be enlisted in the work, and the most ultra should receive them. and then, on a platform judiciously formed, wage a warfare, not in a way that invites disaster, but in a manner which will enlist the aid and support of the masses.

SHOULD IT BE TREATED AS A DISEASE?

A theory is being constantly urged, particularly by *Superintendents and Physicians of Inebriate Asylums*, that "*Drunkenness*" is a *disease*—not a *vice*. Their ideas and peculiar views find ready believers and advocates in the friends of the more aristocratic class of those addicted to the intemperate use of whiskey, and among the victims themselves who do not *care to attempt* reformation. I am not aware that anybody ever thought drunkenness in *a poor man a disease*—it is always a class that are able to pay large fees to specialists, and those able to board at asylums at \$50 per week (more particularly in the East) and coddled with the idea that they have a peculiar pathological condition, that we find the largest number of advocates of this (to me) pernicious and destructive theory.

As early as 1872, I placed on record in an article published in the CHICAGO MEDICAL EXAMINER, certain observations in regard

to the newly discovered disease, inebriety. They were directly and explicitly opposed to the theory, and after nearly ten years' additional experience, during which more than 2,500 cases of alcoholism and 1,500 complications have been treated, my convictions remain unchanged. It is probably just for me to say, however, that a large number of physicians, particularly those engaged in the specialty of nervous diseases, believe alcoholism to be a disease.

I desire at this time to again affirm—

1st. That in my opinion there exists no physical cause which compels a man to drink intoxicating liquors.

2nd. That 99 out of every 100 men who have formed the habit *can* reform if they desire; the remaining one per cent. are probably insane and should not be held accountable. The trouble is, men will not use the means for reformation which are freely and entirely at their command. They desire to reform, they say, and yet insist in indulging in all other previous bad habits and associations. They say they desire to reform but refuse any cultivation of the moral sense, any strengthening of the will. They desire a reformation which costs but little or no effort—a *cure* which will enable them to practice all previous bad habits, mingle with all previous vicious associates, gratify all the lower feelings and passions, and to all of which I might add, use alcoholics with moderation, with no danger of ever becoming intoxicated. This is the kind of cure many desire. They do not want reformation.

I am constantly having gentlemen under my observation, formerly addicted to the use of alcohol, men of great business capacity, men of fine moral sense, and with wills powerful enough to carry them directly in the way of duty, notwithstanding any reverses, disappointments or temptations, and yet they believe in the so-called disease inebriety, and that they are victims of the malady. With these splendid business qualifications, with keen moral and religious senses, and with will powers equal to any emergencies, they clamor for some *specific medication*, which will do away with the appetite, which will enable them to take a single drink without the desire to take another. A man is reformed when his moral sense teaches him that it is wrong, and his will power is sufficiently strong to enable him to abstain from

taking the first drink, and for such a person to assert that he is the victim of a disease is to me nonsense. It should be remembered that we are now speaking of men who have acquired the taste for stimulants, and have a desire for them. We speak of the desire or motive for taking them as the disease, not the condition they are in after the stimulant has been taken into the system.

Dr. Parrish, in the Third Annual Report of the American Association for the Cure of Inebriates, speaking of consumption and alcoholism says: "The cough, hectic flush, night sweats and wasting of tissue are no more symptoms of disease in the one case than the injected and glaring eye, flushed face, confused speech and staggering gait in the other."

To me there is no comparison whatever between the two cases. Place these two patients in a room; supply the consumptive with a good generous diet and what medicine is necessary; supply the patient with the so-called alcoholic disease with a little nerve sedative, some beef tea, etc; visit them at the end of four days, and mark the progress of their respective diseases. The consumptive has continued to fail: his emaciation is greater; his strength is not so good; he is worse. How is it with the other? His injected and glaring eye has disappeared; his face is no longer flushed; his speech is intelligible; his gait steady. In a few days he is ready for business. He will now stop drinking if he earnestly desires. In order to accomplish this, however, he must do what every man must do who is peculiarly tempted by appetite or passion—carefully avoid the temptation, and in every possible way earnestly and zealously perfect a noble manhood, and reformation will certainly crown the effort.

Alcoholics produce very grave changes in almost every tissue of the body, and as I have said, and shall say again, produce disease of every variety. But it is the motive, or impulse, or desire that is called the disease. If a person is not responsible for an evil motive, or impulse, or desire, then we should not hold him responsible for all the calamities which come from partaking of alcohol. A man is not responsible for acts which absolute structural changes in his body compel him to do. I grant this of course; no one pretends that a man is to blame for the palpitation of a diseased heart induced by a rheumatism, or for an en-

larged spleen caused by chronic malarial poisoning. It is beyond the power of the will to prevent these maladies ; they are of the body ; they are diseases ; we cannot avoid them ; they have a previous history and a regular course. There is a cause for the structural changes which really exist.

And now about Inebriety : Is there any previous history ? Any cause except the voluntary taking into the system a certain amount of liquor ? Has any structural change taken place in the persons of the hundreds when the first drink is taken ? Is there any inherent propelling cause why our pastors or our deacons should go out and take enough liquor to render them absolutely unconscious ; and yet pastors and deacons, and doctors do and can, become completely under the influence of some alcoholic in one short hour. Shall we excuse them, and say they are not responsible and call it a disease ? If this is a disease how easy the cure compared with all other forms of sickness ? Take a man who has drunk continually for ten years ; all at once he stops and never drinks again. We know them by the dozens. What has done it ? What has cured the man of this “ disease ? ”—this malady which has beggared his family for years and ostracised him from society ? Is it any drug ? Is it any change in some organ, where structural lesions had existed ? Not at all. *He simply made up his mind not to drink.* Henceforth everything progresses well ; the man is in perfect health ; his business is excellent—never better ; his family is happy ; he conducts large and profitable enterprises ; his mind was never as clear—his reasoning powers never so acute. This man, then, has been cured of his so-called desire without any medication, *by a simple process of his will*, and he remains so to the end of life. No other disease, the history of which I am acquainted, is cured like this. Some may urge, however, that this is an exceptional case ; that it is a recent case ; that the appetite had not obtained control of the man. To this I say, that a close analysis of more than 2,500 cases convince me—that a man who has drank ten, twenty or thirty years stops just as easy as one who has been addicted to alcoholic excess for a single month.

At this stage in the discussion I shall be met with the assertion, that since I maintain that it is not a disease, and inasmuch

as I say that it is all nonsense to talk about the irresponsibility of men in regard to drinking liquor, some will begin to talk about the sins of the fathers being visited upon the children, even to the third and fourth generation—and the anxious and kind-hearted relative and parent commences to excuse the drunkard on account of inheritance. They refer you to the scheme inaugurated by Laban for changing the color of his cattle, sheep and goats, and ask if you deny the fact that paternal impressions and peculiarities are not at the present day transmitted to offspring.

I do not propose to discuss the matter of heredity. It is a topic itself, the consideration of which would consume more space than I have at my command, but I do desire to state a few facts. Of 541 men admitted to the Washingtonian Home during 1880, only eight (8) dared to say that they thought the appetite which they persist in appeasing was the result of hereditary influence : and of 1,525 admitted during the past four years, *only thirty-one* claim any influence of this kind. One other fact I desire to state and then I pass to another point. This is a matter of experience ; a matter of observation. A man is reared in a moral or religious family, where liquor is never seen, and where the influence of heredity can by no possible way exercise any influence, but where indulgence and lack of early discipline is prevalent, and a general easy and indolent time is permitted. Let such a young man acquire the habit of drinking, and his reformation is more difficult than that of a young man coming up in a family where liquor is freely used, but very early he is taught to care for himself, to work, to obey those in authority, and to assume some of the responsibilities of life. That is to say, idleness and want of government are more important factors as causes of drunkenness than heredity.

I regard the testimony of the Bible of considerable importance on this subject. Diseases of various kinds are described to some extent, and vice and wickedness pretty sharply denounced. At no place, as far as I am aware, is a “woe” pronounced on a disease, or its victims precluded from the enjoyment of Paradise. Not so, however, with whisky drinking. The word of God, particularly the New Testament, speaks out in no uncertain manner in regard to the viciousness and sinfulness of drunkenness :

I. "A *woe* on drunkard-makers:" Hab. ii, 15.

II. "A *woe* on the wine-drinker:" Isaiah xxviii, 1, et seq. Prov. xxiii, 29, 30.

III. Drunkenness condemned as other sins: Luke xxi, 34; Rom. xiii, 13, 14.

IV. Drunkenness excludes from Heaven: Gal. v, 19, 20, 21: 1 Cor. vi, 9, 10.

THE TREATMENT OF INEBRIATES.

Having now discovered the first causes of inebriety, and found that a want of education among the youth of our country is the primal element which produces a habit which, in after years, is thrown off with the greatest difficulty; that the habit is formed so insiduously that those mostly concerned are hardly aware of its power before the victim is within its grasp, and *secondly*: having demonstrated by actual observation and by statistics, that after the habit is formed it is nearly impossible for men to entirely overcome the appetite—either from the fact that they cannot be aroused to the importance of accepting all the safeguards which are accessible to all, or because from defects in their early discipline they yield to drink as a panacea for all trouble, and finally, experience having convinced nearly every one having much to do with this unfortunate class, that many do not seem to care for any reform; do not care to stop a habit which not only brings sorrow and degradation to the household, but burdens on the State—what then shall be the treatment?

It seems to me that our labors should be fairly systematized among three classes:

1st. Among the young, who know nothing of the habit.

2d. Among those who seem honestly to reform, and are willing to accept the means which will produce it.

3d. Among the uncontrollable, incorrigible class, who profess to desire a reformation, but, after short periods of abstinence, repeatedly fall, and have not only ruined themselves but have discouraged and rendered miserable their relatives and acquaintances. This class is really a burden to the community.

As a result of my investigations, I am particularly impressed with the idea that the largest amount of temperance

work should be among the young; that by every means possible they should be prevented from forming the habit of *moderate drinking*; that the law against selling liquor to minors should be *rigid and enforced*.

The youth of our country, of all nationalities, should be made to feel that the cultivation of the taste for liquor is a dangerous habit; that alcohol in all its forms is a drug to be dreaded and avoided as strychnine or aconite. Let parents in all conditions of life be made to feel, and educated to believe, that it is a dangerous practice to allow, and a fearful responsibility to encourage their children to drink the mild wines, beer or cider.

While I am writing this section of my paper a gentleman fifty-two years of age, and one who has experienced all the degradation possible from drink; has lost his fortune; ruined his health, and became separated from his family, said to me: "Forty years ago I attended church three times each Sunday, and listened to sermons one hour in length each time, and in addition attended Sabbath school, and during all these religious exercises for years *I never heard a word in regard to temperance*. I commenced gradually and knew nothing of its dangers." This is true of hundreds and thousands. Such a scheme as I have mentioned is, I am happy to say, already being inaugurated and has been pushed forward by some church organizations with more than usual success: but it is not general enough; it reaches only a comparatively small number of those who should be benefited by such efforts.

What shall be done with the class who have already acquired the habit?

Probably about twenty per cent. can be reformed the first time their attention can be arrested long enough to keep them perfectly sober. This can be done by Homes, Asylums, Societies, Prayer Meetings, Ribbon Clubs, etc. Any way by which the whisky can be eliminated from the system, and a few days rest for the body be taken, and then followed by encouraging words; a definite plan of work, and a resolution never to associate with former companions. If the question is asked, Why have homes and asylums for inebriates been established and maintained at a cost of money and labor, if the habit of inebriation is not a disease,

and only twenty per cent. saved? I would reply, that after temperance organizations, and societies, and churches, and clubs have done all that is possible, some of the most promising of those reformed will relapse from causes which I have just enumerated.

These men need a place where they can acquire strength; where, separated from their companions, their *moral sense* can be cultivated and strengthened. Some men reform in a moment; with others it takes months, perhaps years. Many men use alcoholics for years without the impairment of any function of the body; others are injured in nearly every organ. For those who need considerable time to perfect a reformation, and that class, too, whose constitutions have been injured by the voluntary use of alcoholics, and who, from habit and association, have not the moral force to say No to an invitation to drink, to such, homes and asylums are houses of refuge. Reformation is a matter of development—not a “presto-change” affair—not done in the twinkling of an eye—but a matter of growth, a matter of development. A man who has been addicted to the inordinate use of whisky is never safe. He must be on his guard. A reformed man must be taught, and the lesson must be well learned, that let come what will—sickness, trouble, death, reverses—singly or all combined—that he cannot, with any safety, touch a drop, or he will fall.

Men who have stopped drinking, but are not guided by a high motive; those who have not made up their minds that for them it is wicked to use alcoholics because of an acquired taste, will constantly be making experiments. They see the great majority of men drinking a little every day, and they believe that they can do the same. They make up their minds to drink only beer after supper, or mineral water in the forenoon, some cinchona in the afternoon and beer in the evening. They proceed to experiment, and in about 999 cases in a thousand they fall. These men, then, are never safe; their burden is to fight an appetite which was formed before they knew its danger. Asylums for the reception of this second class should multiply, they must be increased, and the work in this generation will never cease. If it becomes less it will be due to the fact that the influence of innumerable temperance educators is being felt by the youth

of this country. Until this is done we must provide for the victim of intemperance.

What is to become of the remaining eighty per cent.? I stated, it must be remembered, that I thought about twenty in each hundred could be quickly reformed, if we could get access to them. What about the remaining 80?

Quite a number will reform after repeated trials and failures; a few, after education upon the subject, will drink less—are improved, but not reformed. But a considerable number go to form the third class, the description and treatment of which I will now speak.

These cannot be reformed, either through the influence of temperance organizations, or by the moral strength acquired in a reformatory institution. These are the uncontrollable, incorrigible, disquieted men, who have not only discouraged but rendered miserable their relatives, and all their acquaintances. They have never learned to obey; they are undisciplined, and generally lack all feeling of responsibility. They are the men who beat their wives and starve their children; the men who steal the hard-earned money from their wives' purse, and the knives and napkins from their tables (as has happened within my own observation within the past two weeks) to buy rum, and, when perfectly sober, you expostulate with them, they will laugh, and assure you that they know their own business. The State should assume the guardianship of this class, and should put them in an institution whose management should be remarkable for its kind administration of affairs, and for a discipline most rigid. This institution should be situated on a farm, and men of this class sent there for not less than two years. From ten to twelve hours work every day during this time of commitment, combined with judicious and strict discipline, with the assurance that the State would again assume charge of them if they returned to their former habits, would produce an effect on these men, which would result in the reformation of nearly all. Let them learn that it is *some one's business* if they become absolutely indifferent to all of the responsibilities of life. In the language of Dr. Bucknill: "These men are not to be 'coddled' in luxurious indolence, nor impressed with the pernicious idea that they are interesting but helpless objects of social and psychological science."

ARTICLE IV.

THE PATHOLOGY OF INVOLUNTARY SPERMATIC FLUXES. A Paper read before the Eastern Kansas Medical Society. By GEO. H. PICARD, M.D., Topeka, Kan.

The subject I have chosen for this paper has by no means been exhaustively treated by American medical talent. Indeed, the prudent surgical *litterateur* chooses rather to pass it by, so malodorous has it become in the hands of charlatans and self-constituted "sexual pathologists." Excepting the admirable monograph of our distinguished countryman, Roberts Bartholow, and an English translation of Lallemand's "*Des pertes Seminales Involontaires*," I know of no work in our language which is exclusively devoted to the subject, which now-a-days seems to be regarded by the surgeon with much the same distrust as the careful husbandman exhibits toward the well directed approaches of the fragrant quadruped scientifically known as *mephitis chiuga*.

For much the same reason, I opine, nothing is more evident than the fact that the general practitioner of medicine and surgery knows very little either of the pathology or treatment of spermatorrhœa; not, indeed, because there is any especial difficulty connected with the proper investigation of the subject—I mean in the learning of whatever is to be taught about it—but rather because the merit to be gained in this direction is so inconsiderable that most men prefer to labor in a field less approximate to quackery and more certain of repute.

As is often the case, the first difficulty in the discussion of spermatorrhœa is to be found in the definition. What is spermatorrhœa? Is it, as the term seems to imply, a constant stillicidium of semen from the external urethra? Or, does it embrace all discharges except those of a known specific origin, which may at any time escape from the urethral orifice? Is it a disease? Is it a symptom? Is it a neurosis? Is it a local lesion? What is it? To say that it consists in a mere passive stillicidium of semen without the accompanying phenomena of

erection and ejaculation would include a collection of instances so rare as to almost do away with the necessity for a definition at all, and very likely exclude a far greater number of cases entirely pathological in character. On the other hand, it is not safe to affirm that every seminal-like discharge issuing from the urethra is actually seminal, for it is altogether likely that most of these fluxes do not contain spermatozoa, and are the result of irritation and hyper-secretion of the mucous surfaces of the canal.

At this point, lest it should appear that I am trying to shirk the responsibility of defining spermatorrhœa, I will give my notion of it, and although I am aware that it is by no means exact, I am persuaded that it includes nearly everything essential to a definition. *Spermatorrhœa embraces all involuntary spermatic fluxes occurring too frequently to be regarded as physiological.*

About fifty years ago M. Lallemand, the distinguished surgeon of Montpellier, expended much care and research in the investigation of the pathology of this disorder. So clever, so lucid, and withal so dogmatic were the results of this scholarly search as given to the world in the work, published in 1837, entitled "*Des Pertes Seminales Involontaires*," that the author's views were almost universally adopted wherever surgery was known. Indeed, at the present time, most English and many continental surgeons cling fondly to the dicta of the surgeon of Montpellier, and I predict a revival of his doctrines in this enlightened country.

In order to make my observations clear, it will be necessary to preface them with a brief *résumé* of Lallemand's theory. The central idea in this surgeon's theory of the pathology of spermatorrhœa is the production, by various causes, of an irritation of the prostatic portion of the urethra and the seminal ducts. The following causes play an important rôle in the instigation of the disorder: Blenorrhagia and stricture of the urethra; gouty and rheumatic affections of those parts; sebaceous accumulations beneath the prepuce; venereal excesses and masturbation; excessive use of diuretics, cantharides, ergot, etc.; the abuse of alcoholic stimulants, and of tea and coffee; constipation ascarides, hæmorrhoids, etc.

M. Lallemand divides seminal losses into *diurnal* and *nocturnal*. Diurnal fluxes, according to the observer, are of vastly less frequent occurrence than those of a nocturnal character, although they are quite as serious, if, indeed, not more rebellious to treatment. They take place, for the most part, passively, without erection or ejaculation, during or just after defecation and micturition. It is not common to find patients suffering from both daily and nightly fluxes. The nocturnal losses are frequently connected with all the phenomena of sexual excitation, becoming pathological by reason of their involuntary nature and their abnormal frequency.

M. Lallemand found the results of genuine and well established spermatorrhœa to be speedily disastrous, the mischief being due to local injuries and important functional disturbances. "The penis becomes relaxed, the erections feeble; the *corpora cavernosa* either atrophy or their vessels lose tonicity, the *corpus spongiosum* and the *glans penis* also shrink; the testes undergo a certain degree of atrophy; the superficial veins of the penis become dilated and tortuous, etc. The subjective symptoms begin after a longer or shorter time to be marked: pains in the lumbar region; aching in the arms and testicles; the appetite is capricious, the digestion feeble; the bowels become deranged, constipation alternating with diarrhœa. The nervous system very often manifests sympathetic disturbance in the forms of vertigo, pains along the courses of the principal nerves, etc.

As the outcome of his microscopical research, M. Lallemand teaches as follows: "When the spermatorrhœa has assumed sufficient importance to influence the rest of the economy, the semen becomes more liquid and the spermatozoa are less developed and less vivacious, although their number may not appear to be diminished. When the erections begin to diminish, the semen is still more aqueous, the dimensions of the spermatozoa are sometimes one-fourth their normal size, their caudal prolongation being hardly distinguishable with a power of 300 diameters." In support of his theory of the local nature of spermatorrhœa, M. Lallemand brings to bear a far greater amount of cadaveric observation than any of his successors, be they

friends or foes, and in a supplemental treatise, published in 1839, he stoutly maintains the correctness of his early views.

Opposed to Lallemand and the Montpellier school are the great clinician Trousseau, in France, the neurologist Rosenthal, in Germany, and our not less distinguished countryman, Roberts Bartholow. Surgeons, as a rule, adhere to the teachings of M. Lallemand, thus resisting the encroachments of the all absorbing neurologists.

In giving a short account of the neuro-spermatists, as I propose, for the sake of convenience, to call all those who believe spermatorrhœa to be a actual neurosis, I shall avail myself of the fascinating monograph of Prof. Bartholow, which has lately reached its fourth edition, an event in medical literature in itself worth recording. "Spermatorrhœa," says Prof. Bartholow, not a whit less dogmatically than Lallemand, "is a neurosis. Although structural alterations may be coincident, they are not causative." That the disorder is a local lesion, Bartholow claims is unsupported by the evidence of morbid anatomy, and he does not hesitate to insinuate that Lallemand's cases were selected and his theory constructed to justify his practice. In support of his opinion, he first cites the authority of Mr. Thompson, who asserts that "sexual indulgence cannot have the effect of producing prostatitis, unless gonorrhœa be already existing." He further places in opposition to the views of M. Lallemand his own personal observations. It appears from his account, that he has lately made the dissection of a young man, dead of a double pneumonia, who was known to have practiced masturbation for many years, (whether or not this young man had spermatorrhœa he does not state). He informs us that he found no lesion beyond a catarrhal condition of the seminal and prostatic ducts and of the *vesiculæ seminales*. On this ground he declares the position of Lallemand untenable. He also maintains that the views of Lallemand in regard to the alterations in the spermatozoids are hardly less fanciful than his ideas on local structural alteration. In confirmation of this, he uses the experience of Liégeois, who out of six cases under observation, claims to have found only one case of azoospermia. Bartholow objects very positively to Lallemand's division of *diurnal* and *nocturnal*. He

regards the existence of passive diurnal emissions to be purely mythical, and explains that the passive discharges which sometimes occur during and after defecation and micturition, are not seminal in nature, being entirely destitute of spermatozoa. In this view he claims the support of Austin Flint* and many other original observers.

In spite of the literary and speculative fascination attached to these recent theories, I am, from a rational standpoint, a firm believer in the correctness of Lallemand's teachings. It seems to me that exact neurology is already sufficiently comprehensive without spermatorrhœa. It is beginning to appear that whatever the ambitious gynæcologist has not already included in his specialty is about to be appropriated by the equally enterprising neurologist. Thus is the once fair domain of the general practitioner being parcelled out among the specialists. That the pathology of spermatorrhœa consists in structural changes, manifested locally in the generative apparatus, appears rational to me for very many reasons.

Prof. Bartholow's charge against Lallemand's sincerity in the matter of the selection of his cases for cadaveric observations seems to me to be trivial and unworthy of Bartholow's great reputation. Lallemand did not base his opinion on a single observation, as Bartholow seems to have done; on the contrary, he deducted his theory of structural change from numerous carefully conducted observations extending through a period of over fifteen years. It is also a significant fact that subsequent observers have confirmed M. Lallemand's assertions, *e. g.*, M. Raige. De Lorme, in his "Dictionary of Medicine," states that "in cases of spermatorrhœa, diverse alterations of the genital organs have been found," and, indeed, the fact has never been disputed until Prof. Bartholow *did not* find structural alteration *in his one case*; and it cannot but be evident that for the value of the evidence it would have been better to have stated whether or no the man had spermatorrhœa.

Prof. Bartholow has no faith in the phenomena of passive

* What Flint really says is this: "In the most of these cases the fluid is either the *liquor prostaticus* or a secretion from the *vesiculae seminales*. The microscope affords the only means of determining that the fluid is seminal."

diurnal spermatorrhœa. He considers the fluxes which occur during and after micturition and defecation to be mostly of a mucous character, and less frequently the pathological relief of certain surcharged and plethoric individuals whose secretory functions are greater than their retentive capacities. In support of this belief, he quotes Dr. Chambers, of St. Mary's Hospital, who states that this discharge "has no more pathological significance than the leucorrhœa of women,"—whatever that may mean. To assert that the leucorrhœa of women is devoid of significance is to fly in the face of modern gynæcology. With a somewhat limited experience—I have a record of five cases of this kind—I am prepared to substantiate the correctness of M. Lallemand's views on this point. It may not prove wholly uninteresting if I relate the history of a case, which, at the time, I regarded as especially illustrative of the matter in question: An American of robust appearance, aged thirty-three years; married seven years; father of one child five years of age; by his own confession, inordinately addicted to venery. About three years ago, he first became conscious that a passive urethral flux followed defecation at intervals of three or four days; suffered no diminution of his sexual appetite for two years, but thinks that for the past year his desires are less importunate; no falling off in weight; no symptoms of nervous disorder; no nocturnal emissions; has experimented as to the effects of living *absque marito*, and finds that the passive fluxes continue and that they are not superseded by nocturnal ones. I have repeatedly examined these fluxes with the microscope and have always found them to be seminal. At this point I may remark, that I have more than once had my faith shaken in regard to the soundness of Prof. Bartholcw's microscopic efforts. He confesses that even in known cases of spermatorrhœa which had existed for years, he has never found any change in the number or configuration of the spermatozoa. For my part, I can very decidedly affirm that I have seldom found the spermatozoids unaltered, the most noticeable change being a shortening of the caudal filament. I have not observed that there is a marked diminution in numbers; but that there is a positive decrease of motion, and that the caudal prolongations of the spermatozoids are very considerably reduced,

is clearly apparent with a microscope of only 300 diameters. I have repeatedly verified this fact and am quite satisfied that it will stand the test of criticism. It cannot be denied that there is a very close connection between the local phenomena of spermatorrhœa and a series of neurotic symptoms which in a large number of cases are likely to supervene. It is equally true that all other local lesions may be accompanied by the self-same nervous disturbances, always bearing in mind the fact, that lesions of the reproductive system—for reasons which are obvious—are less heroically borne than injuries in any other part of the human anatomy. No one can have failed to observe that all chronic disorders are often productive of melancholia and extreme nervous prostration. I have seen as great mental depression arising from the existence of an incurable ulcer on the leg as could be possible even in the most advanced stages of spermatorrhœa. The whole structure of modern gynæcology rests upon the neurotic sequences induced by primary uterine lesions.

It seems to me that the theory of the exclusively neurotic character of spermatorrhœa proves too much. That the nervous system exercises a peculiar influence over the glandular apparatus is undoubtedly true. Is any neurosis causative of prostatorrhœa or blenorragia? Review for a moment the morbid anatomy of spermatorrhœa (according to the Montpellier school, which I prefer by reason of its greater accuracy): There are ulcerations of the orifices of the ejaculatory ducts; injection and ulceration of various portions of these canals; similar alterations in the *vesiculæ seminales*; purulent deposits in the vesicles, in the *vasa efferentia*, in the epidermis, in the body of Highmore, and in the testicle. Now, in prostatorrhœa there is precisely an analogous condition of the ducts; yet, as far as I know, no neurologist has proclaimed the sovereignty of the spinal cord over this disorder. Is not the abnormal increase of function in one series of glandular apparatus as easily to be accounted for as in another?

Bartholow teaches that genuine spermatorrhœa is exceedingly rare, and that most cases that present themselves for treatment are purely imaginary. I believe this to be fortunately true, but does it not somewhat militate against the neurotic theory? If

spermatorrhœa be purely nervous in its origin, would it not, in view of the enormous existing causative influences, be an every day matter? In these degenerate days of shattered nerve forces, habitual excesses and social corruption, one might indeed shudder at the unwholesome possibilities suggested by the neurotic idea! I grant that this theory is excellently well adapted to imaginary spermatorrhœa, but for the pathology of true spermatorrhœa I prefer something more rational.

But the most telling testimony against the theory of Trousseau and Bartholow may be found in its therapeutical sequel. I am not unwilling to admit that anaphrodisiacs may be useful in a placeboic sense, and that nerve-stimulants—so called—may serve to buoy up the despairing fortunes of the hesitating sexual neophyte; but I do not believe that a case of real spermatorrhœa has ever been cured by any one or by all of them. Clinical medicine must have shown this to be true.

A fact worthy to be noticed is, that in spite of the strictly neurotic ideas enjoined by both Trousseau and Bartholow, they rely very greatly upon local and mechanical treatment. M. Trousseau has even achieved his most satisfactory results from the employment of local empiricism, such as the forcible dilatation of the *sphincter ani*, and the insertion of a boxwood block into the rectum. Bartholow does not hesitate to avow his lack of confidence in nerve-stimulation and brain-food; he seems to find his most reliable agent in hydropathy, applied in the form of the continuous cold current. I have not observed that he explains the therapeutics of the continuous aqueous current in any original or strikingly neuropathic manner.

If an account of the rational treatment of spermatorrhœa were demanded of me, I should say: Be certain of the diagnosis,—it is a somewhat rare disorder, and all passive fluxes from the urethra should be doubtful until the microscope has told the story; treat nervous complications as intelligently as you would those arising from any other cause; and use some approved form of local treatment. I agree with Bartholow, that the *porte-caustique* of Lallemand is a somewhat dangerous agent in unskillful hands, but it is very efficient in the hands of the

surgeon who knows the parts and the instrument. The tannoglycerine is about as desirable as the caustic and has the merit of being harmless. I have injected a dram or more of the tannoglycerine—5-5—at intervals of a week or so until a cure was effected. These injections may be made in various ways. The universal syringe can be attached by means of the screw to a silver catheter, which can be introduced into the prostatic urethra. There are hard-rubber syringes especially constructed with long, curved, bulb-tipped nozzles, which answer the purpose most admirably. Various other astringent injections are of great service, the only requisite on the part of the surgeon being ordinary skill and attention.

ARTICLE V.

OUR CALLING. An Anniversary Address, delivered before the Seneca County (Ohio) Medical Society, August, 1880. By F. W. M. SCHWAN, M.D., Poplar, O.

All human knowledge, save that which was strictly essential to the physical wants of man, has had a very obscure and primitive origin; and in every department of science, literature and art, we observe the same varying struggle for existence and progressive development. The conditions and relations of men have called forth the various kinds of knowledge that has sprung up, flourished and decayed, as their several wants have been supplied, or the necessity for them subsided.

Primitive man possessed but few attributes and wants, and these were only such as addressed themselves more especially to their physical understanding, prominent among which was no doubt the avoidance of bodily pain, and a dread of death. But as society advanced these conditions were more fully recognized and appreciated, and methods devised for relief. What these methods were will ever remain an impenetrable mystery, but tradition undeniably points out that pre-historic man possessed some crude means to alleviate suffering.

Passing from conjecture to proof, we find it asserted in the

immortal Iliad, that at the siege of Troy, 1,100 years before the Christian era, the healing art had made considerable progress. Of course no corps of skilled surgeons then existed, but individual personages were frequently called upon to minister to the suffering, Agamemnon was called from the ranks of the besieging host to suck the poisoned wounds of Meneleos and Philoktel, and apply soothing salves. Achilles appears to have been especially qualified in the healing art, which he acquired from the Centaur Chiron. Patrokles had also become celebrated as a physician, and when Eurypelos was struck by an arrow, he removed the missile and dressed the wound with a soothing and healing herb, the virtues of which he had learned from Achilles, and which botany informs us to have been the *Achillea Millefolium*, or Yarrow.

Among some of the nations of antiquity it was customary to delegate persons who claimed to possess especial knowledge of the healing art to minister to the wants of the afflicted, and in Asia, Egypt, Assyria and Greece it was the practice to place the invalids on the street corners of the cities, with a placard fastened to them describing their symptoms and asking advice from any passer-by who may have been similarly affected. In some countries the people firmly believed that any derangement of the vital functions was a wrathful visitation of some deity, and the natural manifestation of disease was regarded as an evidence of displeasure of the offended spirit; and this belief being so indelibly rooted among the superstitious populace it became an easy matter for mountebanks and frauds to announce possession of supernatural powers, and by the practice of mysterious incantations and jugglery inspire faith, and proclaim themselves especially ordained as mediators between the invalid and the deity. If, perchance, the poor sufferer failed to receive the desired relief, it was always attributed to the want of belief on the part of the invalid.

In India and Egypt, the most cultured peoples of antiquity, we find invalids making pilgrimages to the sacred temples of Osiris, Isis, and Serapis. Here, also, we find mediators clothed in sacred vestments interposing for the relief of the pilgrims, before some special deity that was thought to preside over some

particular part of the body, and, if some of our modern specialists could take a flight to ancient Memphis or Thebes they would be surprised to find how little they know of the manifold subdivisions into which the human form could be divided for treatment. From among the Brahmins of India, and Levites of Israel, sprang the lawgivers, physicians, priests and mediators; and it will be recollected that Moses, possessing not only the qualification of a lawgiver, but also that of a physician in an eminent degree, was enabled to give to his people a complete code of political ethics and laws of life, which being divested of their assumed sacredness, not only challenge our admiration but command our respect for their correctness. Solomon, also, gave to the Jews a system of "Laws of Health," and advised the priests to teach the treatment of disease by natural means, but those sacred officials resisted the innovation and had the tablets burned.

In Greece—the cradle of philosophy, of science, of literature and of art—the first grand forward stride in rational medicine was made, and in temples especially erected for the purpose, the god Asklepias and his daughters Hygeia and Pavakoia presided, and restored the invalids by teaching the observance of natural laws, based upon natural phenomena, and the application of practical hygiene in treatment.

The temples of Epidorus were also noted resorts for invalids, and were simply hygienic establishments from which the mysticism of antiquity had not been fully banished.

When Greece had arrived at the zenith of her glory; when Pericles directed her administration; when Herodotus enriched her literature with the splendor of his genius and Phidias inspired her art with the grandeur of his conceptions, when Plato and Socrates electrified mankind with the brilliancy of their attainments in metaphysics, medicine was yet a very little, sickly, weakly babe. But a nation that could bring forth such intellectual giants could not long remain in mental bondage, and gradually, almost imperceptibly, did the gods and goddesses lose their prestige, and, in the time of Aristophanes, we already find a numerous sect, known as Alklepias, who made a practice of observing disease and prescribing remedies; they also transferred the healing art from the sacredness of the temple to the private

dwelling, and divested it of the mystery and jugglery that had characterized it from the beginning of the world. About this time was also established the noted gymnasii, in which were taught the observance of the laws of nature as well as the attainment of the highest physical development. Philosophy, also, was a mighty factor in shaping and moulding the minds of men, and when the great father of medicine, Hippocrates, came upon the stage of action, the world was not unprepared to welcome him. The attainments of this extraordinary man border on the marvelous, and his success was so great that his services were called into requisition not only in his own province but also in Macedonia, Thrace, Athens, Thessaly and Asia Minor. He accomplished a remarkable cure on the Macedonian king, Perdikkos, and restored to health Demokritos, of Abdera, for which he was offered ten talents of gold (nearly \$12,000), but refused the compensation, claiming that the acquaintance of the renowned philosopher and savan richly compensated him. He was also called by Artaxerxes to Persia, which call he refused. With the advent of Hippocrates the calling of the physician assumed character, and the practice of medicine became a distinct branch of knowledge. But Greece had well nigh fulfilled her destiny in the category of nations, and her glory began to decline, and with it were scattered all her brilliant achievements—some to be lost forever, while others were rehabilitated in other lands. From Greece the new art was transferred mainly to Egypt, where the great Ptolemy exercised a patronizing care, and for 300 years new discoveries were constantly being added.

With the decline of Hellenic grandeur Rome became a resort for many of the followers of Hippocrates, but the ignorant populace received them with suspicion and ridicule, and finally drove them entirely out of the country. From this time on until the advent of Julius Cæsar, Rome contented herself with her soothsayers, her jugglers and her sybillic writers, and all there was of the healing art was in the hands of an ignorant and superstitious priestcraft, who created a special deity for every disease, prominent among which were the goddesses Febris and Cloacina, also a famous one named Scabies, from which we conclude that the holy fathers were familiar with the itch. Cæsar

became a patron of medicine and gave it every encouragement, and by the efforts of Claudius Galenus it received a mighty impulse which extended to the fall of that vast empire; but with its disruption the art was again jeopardized,—only a faint spark remaining which floated over, and was fanned into life in the mystic land of the followers of the Koran. From Arabia it found its way into Africa, and from thence across the Mediterranean to Spain, where for a long time it made little progress. In Central Europe, now the center of medical science, no art, no science, no culture, found congenial recognition. The refinement of Hellas, of Alexandria, of Rome, was supplanted by the migrations and devastations of the Goths, the Vandals, the Franks and the Lombards. Brute force swayed the destiny of Europe, and for 600 years every possible obstacle was thrown in the way of rational medicine. What little there was of the healing was completely under ecclesiastical influence, and, at the time of Innocent III, in the twelfth century, no physician was allowed to visit a patient without first going to confessional. But an art so intimately associated with the happiness and welfare of humanity cannot always lie dormant even amidst the grossest ignorance and superstition, and with the establishment of the school of Salerus the physician again rose into prominence, and the teachings of Hippocrates, after many vicissitudes, received a new impulse, gathering force and power, continually, down to our own time, resisting all antagonisms and adverse circumstances, triumphing over all opposition, and standing to-day, an enduring monument of humanity's highest, noblest and best impulses and sympathies.

ARTICLE VI.

CREMATION. By Dr. C. W. PURDY, of Chicago. (Concluded from page 200, Aug. No., 1881.)

In 1850, a Report on a General Scheme for Extra-mural Sepulture was presented to both houses of Parliament, by Carlisle, Ashley, Chadwick and Smith. The report contains an account of a large number of London church-yards, showing their bad condition

and the remarkable prevalence and virulence of cholera in their vicinity. Dr. Sutherland testified "that he had witnessed several outbreaks of cholera in the vicinity of grave-yards, which left no doubt on his mind as to the connection between the disease and such local influences; and that it is only when some epidemic comes that the consequence of long antecedent neglect becomes so apparent as to direct attention and excite alarm."

Burial grounds are unquestionably ruinous to health, as both theory and facts amply demonstrate; many sections of population suffer annually diseases and death, which are exposed to their influences; all engaged in this unwholesome system suffer—the grave-diggers, the gardeners, the men who repair the vaults and tomb-stones, the friends who visit the graves, and the whole funeral procession are exposed directly. There is no redeeming feature about this burial system, degrading to the dead and dangerous to the living.

Under the general adoption of cremation of our dead all this would be changed. The body would be quickly, cheaply and harmlessly rendered back to atmosphere and earth; to the former, carbonic acid and watery vapor; to the latter, ammonia and earth salts;—nothing is left corrupt, but rapidly cleansed and purified. Instead of the damp, cold shroud of a decaying grave, shared by worms and reptiles the most loathsome, the atmosphere of light would enshroud the etherialized remains in the brightness and purity of living day.

There is one feature of the burial system which we have but time to notice briefly in passing,—the most shocking to our feelings and dreadful to the imagination. We refer to the awful possibilities of living burial. This has happened, and it will happen. Hardly a grave-yard is opened but coffins are found containing bodies not only turned, but skeletons contorted in the last, hopeless struggle for life underground.

The *London Times*, May, 1874, says: "In August of 1873, a young lady died soon after her marriage. Owing to the intense heat she was buried the same day. Within a year her husband married again, and the mother of his first bride resolved to move her daughter's body to her native town. They open the vault, and find the poor girl's body fallen prostrate, her hair dishevelled,

her shroud torn in pieces. She had risen, and burst her narrow prison to find herself in one a little larger, but far more inexorable. How long did she cry piteously for help? How long did she struggle and pray? We cannot tell: we can but guess her agonies!"

Much might be written on the subject of contamination of springs in the neighborhood of grave-yards, and there is no limit to the poisoning of such waters. One author states that there is not a pure stream of water throughout the whole of England. Is it any wonder then that that country is such a hot-bed of typhoid fever.

A fearful epidemic decimated the villages of Rotondella and Bollita, which was clearly traced to the water which had been drained into the wells through the neighboring hill-cemetery. Hundreds of wells have been found tainted so badly from the same cause as to necessitate their disuse. At Paris, M. Duchamp discovered a spring that filtered entirely through cemeteries and tasted strongly.

Dr. Pappenheim declares that springs tainted with organic matter of grave-yards are often found at great distances from their sources of pollution. But there is no necessity of multiplying instances of a fact that has come under the notice of the most of us.

Let us turn from the dark and gloomy picture of inhumation, whose perpetuated shadows darken the hours of so many happy homes, to the brighter, because purer, more beautiful, because more human, system of cremation.

Of late years a tide of opinion seems to be setting in in favor of the old custom. For the last ten years many distinguished physicians and chemists of Italy have warmly advocated the general adoption of cremation. In 1874, a congress was called to consider the matter at Milan, and petitioned the Chamber of Deputies for a clause in the sanitary code, permitting cremation under supervision of the syndices of the code, which was granted, and the result has been so far successful, that about seventy bodies were cremated in Milan in 1880.

In Switzerland, Dr. Ercolani is the champion of the cause, and there are several associations for its support. So long ago as

1797, cremation was seriously discussed by the French Assembly, and the events of the Franco-Prussian war have again brought the subject under notice of the medical press. Vienna has made cremation permissible, and cremation is practiced in the Austrian Capital. Dresden, Leipsic and Berlin are the centers of the German movement, which is quite as progressive in the matter as Italy.

In England, Dr. Lord, for many years medical health officer for Hampstead, continued to urge the practical necessity for the introduction of the system. Sir Henry Thompson, however, first brought the question prominently before the public, and started in 1874 the Cremation Society of London, the objects of which were stated to be to introduce through the agency of cemetery companies, municipal authorities and burial boards, some rapid process of disposing of the dead, which could not offend the living and should render the remains innocuous."

Thompson used one of Siemen's furnaces, consisting of—

1. A generator, in which the coal or wood generating the gas mixed in certain proportion with the air.
2. A regenerator or square chamber, intersected with perforated brick walls, which receives the gases at a high temperature.
3. A calefactor or actual combustion room; this chamber is flooded with a gas and air at a temperature of 2000° F. It contains a cylindrical vessel about seven feet long, by five feet six inches in diameter; into that vessel is deposited the body, the whole at a white heat.

The gases of combustion rush off but are not allowed to taint the atmosphere; they are caught and carried over irregularly disposed layers of heated bricks till oxidation is complete. Not a particle of smoke at length arises from the chimney, and nothing but a little harmless gas reaches the outer air; throughout nothing repulsive is smelt or seen.

Prof. Gorini estimates the cost of cremating an ordinary body at from \$10 to \$15. The expense attendant on reducing a body in the furnace of Dr. Le Moyne, at Washington, Pa., is \$15. The actual cost of incineration in the Siemens regenerating furnace is only 75 cents. These estimates are for single bodies, which would become very much reduced by the general adoption of the system, as will appear obvious.

The first body that was cremated in the United States, according to Dr. Le Moynes, was Colonel Henry Laurens, a member of the military family of General Washington. His body was cremated in South Carolina, in 1796. The second was that of Henry Barry, who lived and was cremated in the vicinity of Marion, South Carolina. The third was the body of Baron de Palm, cremated in the furnace erected by Dr. F. J. Le Moynes, in Washington, Pa., December, 1876. The fourth was that of Dr. Winslow, of California, cremated in Salt Lake City, in a temporary furnace erected through his request by the administrators of his estate. The fifth was the child of Julius Kircher, in his furnace in New York City, in the autumn of 1877.

During the first two years after the erection of the Washington Crematory nearly a hundred applications were made for cremation, which were declined, for the reason, as Dr. Le Moynes says, "that it was not constructed to be operated as a business, and an occasional cremation only was permitted for the purpose of keeping the subject before the public eye." I am informed however, by Mr. J. V. Le Moynes, that, since his father's death, the executors of the estate have thrown open the crematory for the use of those who desired to be cremated at the cost of incineration, and accordingly some sixteen bodies have been cremated in the Washington Crematory during the past year.

The process of disposing of the dead by cremation, as proposed and recently advocated, is not destruction by fire, or burning in the sense that many suppose. The body is reduced to ashes by the chemical action of intense heat, at a temperature of 2,000 to 3,000 degrees, without contact with flame or fire.

As an illustration we quote from one of the daily papers, observations taken at the cremation of the wife of a German physician in Dresden, November 6, 1874: "The hall around the furnace was decorated with flowers, and in every other respect the solemnity which should attend so serious a site was duly observed. The process of cremation was screened from the eyes of the lady's friends by an iron door. There was no smoke nor any unsightly transformation of the body. When the coffin was consumed, the body appeared in its natural state, then red-hot, and

at last appeared to be of translucent white. From this it crumbled into ashes. Up to the period of its entire consumption, the process was merely a rapid drying up."

In the ordinary Siemen's Regenerating Furnace (used by Reclam in Germany, and Thompson in England), only hot blast is used, the body supplying the hydrogen and carbon, or a stream of heated hydro-carbon, mixed with heated air, is sent from a gasometer supplied with coal or wood, the chamber being heated to a high degree before cremation begins. The advantages of this furnace are, that the heat of the expended fire is nearly all retained by the regenerator, and the gas retort admits of production being stopped without much loss. The noxious gases which are produced during the first five minutes or so of combustion, pass through a flue into a second furnace and are entirely consumed. Bodies may be reduced in this furnace in an hour or less to from three to six pounds of pure white ash, or lime dust. Thompson reduced a body weighing 144 pounds in fifty minutes to about four pounds of ash.

Hawes describes the cremation ceremony, as proposed, in the following language: "The body is conveyed to the crematory and has been placed in its shroud, or, if you will, even in the coffin, at the mouth of what seems to be a small cave at the entrance of a Gothic edifice; the door is closed, and whilst the mourners enter the funeral chapel to hear the service read, the body glides into the crematory by the simplest machinery, and is returned in the words of your burial service: "Dust to dust, ASHES to ASHES," words culled from the old Greek or Roman cremation ritual, misapplied for centuries to Christian burial." No slowly putrefying form remains in the damp, cold ground, with its dreaded memories to haunt the imagination and sting the sensibilities of the living.

Cremation seeks not, as many suppose, to divorce the revered memories or tender affections from the honored dead; but rather it would rob the last resting-place of "the small cold worm that fretteth the enshrouded form." It would separate from the dead only those memories which fill the breast with horror, and feed the imagination with dreadful forebodings of the corruptible before it "put on incorruptible."

Plant still if you will in the sublimated ashes of the departed, the myrtle and the ivy, the primrose and the laurel, whose budding foliage shall cluster about the golden urn and form a perfumed veil of beauty, at once an emblem of the purified remains and a beautiful allegory of the hereafter.

It has been argued against cremation that it interferes with the doctrine of the Resurrection. If this were true, what becomes of the army of martyrs who have perished by fire, and many thousands of good and worthy people who have been burned in conflagration? Did not David thank the people of Jabesh-Gilead for burning Jonathan; nay, did not God himself command Abraham to offer up his son Isaac on the altar, and though he stayed his hand, requiring not the sacrifice, yet the funeral pile and its contingencies were fully authorized by God himself.

But granted even the old doctrine of a literal resurrection, and cremation does not affect it, for a power capable of restoring and refashioning atoms dispersed through the metamorphosis of centuries, would be equally capable of restoring the atoms dispersed by fire in a brief hour.

A more reasonable objection has been urged by opponents of cremation, on the grounds of depriving the courts of means which often convict criminals in poisoning cases. The many great advantages of adopting cremation as a general practice are so weighty as vastly to counterbalance these. Indeed, we have not in man's history any great benefit resulting from a system or practice, but it is attended by its consequent minor evils; no great public good but has its attendant draw-backs. Who, for instance, would prohibit public festivals and holidays, because theft, debt and drunkenness are increased thereby? Who would stop the sale of weapons to prevent suicides; yet the advantages claimed for cremation have greater demands on public policy and safety than most of these.

Not the least objection to the present system of inhumation is its enormous attendant expense, a tax so great on the resources of many poor families, that it actually amounts to oppression. The savings of months or years, which perhaps was the only reliance against temporary reverses or actual want; and perhaps might have kept starvation from the door, is swept away by the

almost absolute necessity of keeping up the appearances of what are termed a decent Christian burial. We have no hesitation in saying that the vast sums of money spent on funerals would keep every needy family in the land from hunger and want. The practice of general cremation would dispose of the dead more in keeping with the laws of nature; more safely to the dead and to the living; in short, more desirably in all respects at a cost less than one-fifteenth the sum now squandered to embellish the earth as a dead-house.

Figures are strong facts. It costs Chicago \$1,000,000 annually for funerals alone, and it cost Illinois, in 1880, \$6,000,000 for the same purpose, which is nearly one-third the amount of internal revenue yielded by the State for the same period. It costs the United States over \$100,000,000 annually for funerals, and the saving which would result from generally adopting cremation would amount to a sum sufficient to pay off our national debt in about fifteen years; while our government has not been able, with the immense revenue yielded by this country, to reduce one-third of this debt in the same length of time. One and one-fourth times more money is spent annually on funerals in the United States than the government expends for public school purposes. Funerals cost two and a half times more money annually than would buy the grounds, buildings and apparatus of all the universities and colleges in this country. The amount of money expended on funerals in the United States, if saved for a fraction over four years, would equal the total sum disbursed by our government for pensions during the last twenty years.

Funerals cost this country in 1880 enough money to pay the liabilities of all the commercial failures in the United States in the same year, and give each bankrupt a capital of \$8,630 to begin business again. Funerals cost annually more money than the value of the combined gold and silver yield of the United States in the year 1880. The population of the Christian world, in round numbers, is about 400,000,000 of people, or a little less than one-third the entire population of the earth. At the average cost of burying the dead in the United States the Christian world would spend annually for funerals \$800,000,000 at a safe estimate. Let this annual expenditure of money be

saved, with its accrued interest at six per cent., for little over twenty years, and it would pay off the combined national debt of the world.

What a vast amount of want and misery the judicious expenditure of this money would save the human race! It would wipe out pauperism from civilization. Yet this does not by any means express the gigantic expense of the burial system, for, remember, we have only now spoken of funerals. Add yet to these the sums of money invested in the millions of acres of valuable property used for burial-grounds; the immense sums lavished in erecting costly monuments and embellishing graves; the depreciation in the value of residence property in the vicinity of these graveyards. Add these together, and we have a sum expended in maintaining these cities of the dead so stupendous, that it is well calculated to arrest the attention of those whose business it is to deal with questions of public policy.

And to what useful purpose to the dead, or the living, is this vast fund so recklessly—I would say, criminally—squandered? To the former, to preserve, against nature's laws, the putrid relics of organic man, in his most humiliating and degraded condition, long years after nature's laws would have transformed him into many varied and lovely forms of renewed organic life, surrounded by sunshine, and giving an air of life and beauty to the grandeur of nature's habitations.

To the living, the expenditure of this sum means creating and perpetuating diseases the most virulent and malignant; the choking of our atmosphere in many instances with gases the most dangerous to health and to life itself; the perpetuation of memories the most ghastly and revolting to sensitive natures; for such never fail to make themselves a part of that vaunted sentiment surrounding the Christian tomb. It means hunger, want and despair, oftener than the world cares to investigate, to many of the poor survivors, whose last savings have been swept away—aye, and heavy debts contracted in order to afford the deceased member of the impoverished family a “decent, Christian burial.”

CONCLUSIONS.

I. That the present system of burial is attended with danger to the living.

II. That abuses of the system are frequent, and at all times difficult to prevent, and sometimes become so serious as to result in overwhelming whole communities with pestilence and death.

III. That a residence near crowded grave-yards predisposes to diseases characterized by evidences of slow poisoning.

IV. That the system of burial, as now practiced, is necessarily attended with enormous expense, which is an oppressive drain on the resources of the poor.

V. That the general adoption of cremation would dispose of the dead rapidly at a nominal expense, compared with inhumation, and with entire sanitary safety to the living.

VI. That a similar disposal of the decaying organic products of large cities would tend to diminish the whole class of zymotic diseases which at present is the cause of considerably more than one-fourth of our death-rate.

ARTICLE VII.

THE PATHOLOGY AND TREATMENT OF YELLOW FEVER; WITH SOME REMARKS UPON THE NATURE OF ITS CAUSE AND ITS PREVENTION. By H. D. Schmidt, M.D., New Orleans, La. (Continued from page 178, August No., 1881.)

REMARKS ON THE PROBABLE NATURE OF THE INFECTIOUS POISON OF YELLOW FEVER.

In the preceding part of this treatise, I first conducted the reader to the bed-side of the yellow fever patient, in order to take a general view of the clinical symptoms, such as they generally present themselves to the physician during an epidemic. After the study of these phenomena, I conducted him to the dead-house, and pathological laboratory, where I showed him the pathological changes in the organs and tissues which had taken place during the course of the disease; and lastly, attempted to draw a sketch of the complex of pathological processes constituting the disease, by comparing the clinical phenomena with the pathological facts revealed by the autopsy and microscopical examination, the whole representing the general pathology of yellow fever.

In discussing the latter, I frequently referred to the influence or effect of the infectious poison, the original cause of the disease, but forbore to make any remarks upon its particular nature, which I shall now discuss. While in the preceding pages, however, we dealt with substantial, incontestable facts, with phenomena or things of a tangible nature, directly appreciable to our special senses, we now approach a subject which, on the contrary, is, to a certain extent, hypothetical in its nature, and where, unfortunately, too much space is left for the vagaries of the imagination. With the exception of cholera, there exists no disease of which the original cause or contagion has given birth to such a variety of speculations, both among physicians and laymen, as yellow fever. Inorganic and organic nature, the earth, the water, the atmosphere, minute plants and animals, the planets, and even invisible germs, have been suspected to be the cause. And accordingly, especially during an epidemic, when the daily newspapers are pregnant with yellow fever literature, there is no want of suggestions, and wise speculations, relating to that mysterious agent, the "germ" of the disease. In such times, high or low, wise or ignorant, everybody has an opinion, a view of his own on the subject, and without regard to whether it is based upon sense or nonsense. How cheaply feels the man of science and facts, the lover of truth and reason, in such a time. His voice is too weak to be heard in the turmoil, and all he can do is to observe and to collect, and to study the facts relating to the subject, to be conscientiously compared with those already established by other observers, at a time when the storm has passed, and the general excitement is worn out.

These remarks will not only show the difficulty, but, moreover, the delicate nature of the subject which I now propose to discuss; and if I fail in this task, it will not be owing to a lack of desire of presenting the subject fairly and without prejudice, but to my incapacity.

In the discussion of a subject, such as the probable nature of the infectious poison of yellow fever, of which the opinions of physicians differ so widely from each other, it is essential that the author should present it to the reader in all its various aspects, and be concise and definite in the terms of his demon-

strations; while, on the part of the reader, in order to properly understand the arguments brought forward and to judge correctly, it is no less important that he should be familiar with the details of the subject. To comply with these propositions, it becomes necessary to introduce the subject with some general remarks.

As long as yellow fever has been known to the civilized world, a controversy has existed among medical men, especially among those of prominent seafaring nations, as to whether this disease is communicable from individual to individual, either in a direct or indirect manner, or whether its cause exists uniformly throughout the air of the infected district, having access to all persons alike. In other words, the question was, and still is—Whether yellow fever is a *contagious*, or a purely *infectious* disease.

As the terms “infection” and “contagion” have formerly been, and perhaps still are, by a number of physicians looked upon as synonymous, though they really convey different ideas, some explanation is demanded in what sense they will be used in this treatise. As far as I understand their proper meaning, the term “infection” signifies that the disease has been acquired by some noxious body taken into the blood from *without*, and without regard to whether the infectious poison is a product of the animal organism itself, or, on the contrary, a body *totally foreign* to it, which, whether *animate* or *inanimate*, owes its existence to external causes, whatever they may be. The term “contagion,” on the other hand, implies that the *active poison emanated* from the diseased person or animal, and, if entering the blood of a healthy individual, gives rise to all the symptoms and organic changes by which the original disease is characterized.

Now, it is obvious that a purely *infectious* poison, as, for example, that causing miasmatic fever, hardly ever exists limited to one particular spot or center, but is rather distributed throughout the whole air of the infected district, and affects many persons at the same time; while a *contagious* poison, like that of small-pox, being *most probably* a product of the diseased organism itself, originally emanated from the affected person or animal, and extends its noxious influence by being inhaled or swallowed by other individuals in the vicinity. From this common center the disease spreads, either *directly* from individual to individual, or

indirectly by adhering to surrounding objects, such as clothes, bedding, furniture, goods, etc., upon which it may be carried to distant places, and, as in the first instance, propagate itself by entering the system of other individuals.

From this explanation it will be seen that some poisons may, in entering the organism, give rise to certain diseases not communicable from individual to individual, and be therefore purely *infectious*; while others, producing communicable diseases, and being *contagious*, are at the same time *infectious*, as they must necessarily enter the blood of the individual before they can exert their noxious influence upon the organism, and manifest their contagious nature. But notwithstanding this general trait of character, possessed alike by the *purely infectious* and *contagious infectious* poisons, the latter have a *special* trait besides, consisting in a certain permanency of the impression which they make upon the organism, and by which the latter acquires the so-called "*immunity*" from a second attack of the disease. Accordingly, a *purely infectious* disease may recur quite frequently, while the first attack of a *contagious infectious* disease protects the organism from another.

Although many causes have in the course of time been assigned to the origin of the various infectious diseases, nothing definite is as yet known about the intimate nature of their infectious poisons. The most prominent theories, at present entertained on the subject, are—the "gaseous," the "glandular," and the theory of the so-called "*contagium vivum*." The first refers the poison to gaseous or molecular substances, arising from the decomposition of vegetable and animal matters, or even from certain emanations from the ground of certain localities, known as *miasmatic* or *malarial* poisons. The second theory explains the poison as being of an animal origin, similar to the poison of snakes or other animals, or produced by certain morbid changes in the blood and tissues, called forth by its original introduction, and giving rise to morbid secretions possessing its specific properties. The theory of the "*contagium vivum*," lastly, refers the poison to certain minute organisms, which, entering the system through the lungs, alimentary canal, or even wounds, get into the blood, and give rise to the specific symptoms of the disease. Each of these theories, of course, exists in different minor forms or variations.

Now, as regards the cause of yellow fever, each of these theories, with its minor variations, has been put forward for its explanation, but without any palpable or positive proofs; for if anything regarding it can be proved, it is only negatively. With a considerable number of physicians, the theory of the contagium vivum, or so-called "germ or bacteria theory," being of a more recent date, has become especially popular. But as its origin and details are, as it appears, least understood of all others, I shall give it the preference for a full discussion, from which the reader may judge for himself whether it is applicable to yellow fever or not.

The theory of a "contagium vivum," relating to the cause of infectious diseases, is not of a recent date, but seems to have existed as a vague idea since the invention of the compound microscope, by means of which the existence of minute organisms first became known to the physician. At the same time another idea prevailed, according to which infectious diseases depended upon a certain process taking place in the blood, similar to that of alcoholic fermentation. These ideas assumed a more definite form by the discovery of *Schwann*, that the yeast-cell played an active part in this fermentation. Nevertheless, the germ theory, thus formed, lingered for a number of years, until it received a fresh impulse by *Pasteur's* proclamation, that every fermentative process was called into action by the agency of minute organisms performing the part of a ferment, and that each kind of fermentation depended upon a special kind of fungus. *Pasteur's* views, of course, were in opposition to the physico-chemical theory of *Liebig* then prevailing, according to which any organic matter, while in a state of decomposition, was able to induce molecular changes in another unstable, fermentable substance. Now, although the assertions of *Pasteur*, that all fermentations are accompanied by living organisms, have been generally admitted as a fact, the question, whether these organisms are the direct cause of the molecular changes, or whether they are to be regarded as epiphenomena, remains as yet undecided, and the theory of *Liebig* is, in a slightly modified form still upheld by a number of the most eminent chemists.

The doctrines of *Pasteur* were eagerly received by many physicians, and applied to the explanation of the cause of infectious

and contagious diseases. The greatest impulse, however, which the germ theory received, was the discovery and description of minute organisms in the blood of animals affected with malignant pustule or splenic fever, by *Pollender* and *Brauell*, in Germany, a subject which, several years later, was more closely studied by *Davaine* in France.

A still more definite form the germ theory received by the examinations of cholera-stools, made by *Klob* during the epidemic prevailing in Europe in the years 1865 and 1866. In examining, namely, the intestinal contents of cholera patients, he discovered a large number of very minute spherical bodies, together with others of a more oblong form, adhering to the outer surface of the epithelial cells. In the intestinal mucus, or in that of the cholera ejections, he also met with these bodies, collected in circumscribed patches in the form of so-called colonies, and imbedded into a gelatinous material. In certain parts of these colonies he observed a number of these bodies, liberated by the liquefaction of their gelatinous envelope, in active motion; while others, united in pairs, or adhering to each other in the form of longer or shorter rows, remained motionless. In the intestinal mucus he met with such colonies formed by oblong, rod-like bodies, either in pairs or, like the former, united into chains of different length.

From these observations, *Klob* drew the conclusion that all these bodies represented but a single organism in its different phases of metamorphosis, and by further inquiry found them, especially the rod-like form, to correspond with the *Zoglæa termo* of *Cohn*, or *Bacterium termo* of *Dujardin*. Meeting, however, with no other vegetable organism in the ejections, but always with large numbers of these bacteria, he concluded that these forms of fungi, constantly met with in the alimentary canal of cholera patients, never attained a higher development in this locality.

From this it will be seen that these bodies represented the same organisms always met with in stale urine, and other putrid substances, formerly known to physicians under the name of "vibrios," though the knowledge of their true nature and development, and the prominent part which they play in the process of fermentation, belongs to more recent times. They were already

known to *Ehrenberg*, who regarded them as animals, and classed them with the *Infusoria*; but when their nature was rendered doubtful, *Haeckel* placed them, about thirteen years ago, in his newly established natural kingdom of *Protista*, composed of a number of other organisms of doubtful origin. *Naegeli*, a distinguished German botanist, formed them into a separate group of fungi, which he named *Schizomycetes*. In classing these organisms with the lower forms of fungi, however, a difficulty was encountered in the want of similarity regarding the precise mode of their reproduction, for while, with the exception of *Cryptococcus cerevisiae*, or yeast plant, in all fungi certain typical organs of reproduction are known to exist, none could be demonstrated to exist in the schizomycetes. For, in the latter, the only mode of multiplication observed, consisted in a division of the minute rods in only one direction, while in the fungi, which are likewise represented by linearly arranged cells, the process of multiplication takes place by gemmation, not only in a straight line, but also by the formation of lateral branches. Finally, after having been studied and classified by other distinguished botanists, a second mode of reproduction, to be mentioned hereafter, was discovered in these organisms only a few years ago by artificial cultivation. In accordance with the importance they gained, their nature and their relations to organic substances were studied more closely, leading to the discovery of a large number of new species, and rendering the field they occupy so extended as to form at present a special branch of study in natural history. They have also been subjected to new classifications, especially by *Billroth* and *Cohn*, too extensive to be followed by the practicing physician. But, as in a few instances, particular forms of bacteria are found associated with some special pathological processes occurring within the animal body, and also with the different kinds of fermentation observed in fermentable substances, I deem it proper to give a short sketch of the lower fungi in general, and of those species of the schizomycetes in particular, which have hitherto been observed in association with these processes.

As regards the lower forms of fungi in general, *Prof. Naegeli*,*

*C. v. Naegeli. *Die niederen Pilze in ihren Beziehungen zu den Infections Krankheiten und der Gesundheitspflege*. Muenchen, 1877.

of Munich, one of the first authorities in Botany, divides them in three natural groups, as follows :

1. *Moulds*, representing branched, segmented or unsegmented filaments.

2. *Sprouting fungi* (yeast-cells, *saccharomyces*, *mycoderma*, etc.), representing spherical or oval cells, multiplying by means of sprouts from their surfaces, and living either singly or united to form arborescent colonies.

3. *Cleft fungi* (*schizomycetes*, putrid-fermentation cells, *micrococcus*, *bacterium*, *vibrio*, *spirillum*, etc.), are spherical cells multiplying by division, and met with either singly or united into rows (rods, filaments), rarely into cubes. They represent the smallest organisms known, the weight of 30,000 millions of the smaller forms would scarcely amount to one milligram.

“The moulds slowly destroy organic substances by consuming them for their nutrition, while the sprouting and cleft fungi act besides as ferment in decomposing large quantities of matter by fermentation.”

“The moulds and sprouting fungi are closely related, though, with the exception of one case in which the same fungus (*mucor*) appears in two forms of vegetation, they have as yet not been directly traced from one form to another. The cleft fungi, however, stand in no genetic relationship to the two other groups, as they can neither produce other forms of fungi, nor take their origin from these.”

“The natural species of the lower fungi are not developed in such a manner as to have particular functions of decomposition corresponding with them. If there are different species existing among the cleft fungi, then each of them will cause different decompositions, as, on the other hand, the same decomposition may be caused by different species.”

The following classification of the *Cleft fungi* or *Schizomycetes*, will suffice for all practical purposes.

The most simple form in which bacteria are met with is that of minute spherical or oval granules or cells, designated by *Cohn* the *sphæro-bacteria*. These are identical with the *micrococci* of *Hallier*, measuring from $\frac{1}{1000}$ to $\frac{1}{1200}$ mm. in diameter, and are often found aggregated into so-called “*zooglea*” or colonies, held

together by a gelatinous intercellular substance, or, also, forming bead-like chains. They are motionless, and are sometimes found in putrefying substances in company with other bacteria, and and have also been considered identical with those granules found in the vaccine lymph.

The next group is represented by *bacterium termo* and *b. lincola*. The first presents the form of a minute rod or dumb-bell, has a slow, vacillating motion, and measures from $\frac{1}{360}$ to $\frac{1}{480}$ mm. in length; it is always found associated with putrefactive fermentation. The other is somewhat larger, and exhibits more rapid movements, being found, in company with *b. termo* and other forms, in stagnant water, as also in putrefying infusions.

The members of the third group are distinguished from the true bacteria by adhering to each other in the form of chains or "linked rods." It consists of *Bacillus*, representing a transversely lined filament, and of *Vibrio*, appearing as a cylindrical and curved filament. There are three species of *bacillus*. The first is *b. subtilis*, a slender thread, about $\frac{1}{20}$ mm. in length, and exhibiting a pausing motion. The second, *b. anthracis*, or *bacterium carbuncolare*, represents a motionless, oblong, and highly refractive body, found in the blood of animals affected with splenic fever, and measures from $\frac{1}{400}$ to $\frac{1}{80}$ mm. in length. The third is *b. ulna*, a stiff filament of $\frac{1}{26}$ mm. in length, and of greater thickness than *b. subtilis*. The *vibrios* are distinguished from the preceding forms by their rotary motion. The *v. regula* represents a flexible thread, from $\frac{1}{100}$ to $\frac{1}{43}$ mm. in length, distinguished by one or two slight curves and its slow motions. The other, *v. serpens*, measuring about $\frac{1}{80}$ mm. in length, is thinner than the preceding, and distinguished by the greater number and regularity of its curves and its more rapid serpentine motion.

The fourth group consists of the *spiro-bacteria*, distinguished from the vibrios by the greater regularity and closeness of the curves of the spiral, and their uniform, cork-screw motion. They are represented by *spirochaète*, to which the spirilla, found in the blood of relapsing fever patients, belong; and by *spirillum*, fine threads of a cork-screw form with a rapid spiral motion.

Nearly at the same time with Klob. E. Hallier, a professor of Botany in Jena, had also made a number of microscopical exam-

inations of cholera stools, and for some time agitated the medical world by the discoveries he claimed to have made, consisting in certain spherical and oblong vesicles, filled with minute yellowish and glistening cells or spores, and also of certain spherical masses of minute roundish bodies, held together by a homogeneous gelatinous substance. To the granules or cells of these masses (colonies), adhering in large numbers to the remains of food and to epithelial cells, Hallier applied the name of "Micrococcus." Obtaining almost the same results from other examinations of cholera stools and vomited matters, sent to him from other places, he undertook, by certain methods, to cultivate the fungous elements they contained; and after the lapse of several days, he really found an increase in the forms of micrococcus, cryptococcus, torula, and leptothrix. In some of these cultures only certain forms of fungi appeared, while in others, other forms were observed, such as *oïdium lactis*, *mucor racemosus*, and also *penicillium glaucum*.

These examinations, made upon old material, could not have, as will be readily understood, any important bearing upon our knowledge of the cause of cholera, and for this reason could not but meet with much opposition and criticism on the part of other investigators. Nevertheless, Hallier drew from these observations the conclusion that those minute granules of micrococcus were able to give rise to various forms of fungi, and, moreover, that the particular form of a fungus depended upon the particular material in which it was cultivated. Thus, from each species of fungus micrococcus spores might be derived, which, according to the nourishment they received, would be developed into one or the other species of fungus.

Thus far, the whole question concerning the metamorphosis of one species of fungus into another would have remained botanical in its nature, if Hallier had not extended the bearing of his observations to the field of medical science in asserting that all infectious diseases were caused by the presence of micrococcus spores penetrating into the capillaries of the human organism, in which they multiplied, without ever being developed into a perfect form of fungus. Accordingly, each form of micrococcus would give rise to a particular disease. The same theory he ap-

plied to the process of fermentation, in which, according to the nature of the fermentable substance, a different fungus was produced. The application of Hallier's fungi theory to the causes of infectious diseases met with considerable opposition in Germany; and being finally rejected, made room for the so-called "bacteria-theory," which, though *apparently* supported by a few observed facts, to be discussed directly, is still very far from being established as a truth applicable to all infectious diseases.

Let us now take a glance at those diseases in which bacteria are almost constantly met with in the blood of the affected individuals. There are, in reality, only two in number—viz., splenic fever, and typhus recurrens, or relapsing fever,—to which may be added purulent septicæmia, and pneumo-enteritis of the pig, described by *Klein*. In the latter, however, *no bacteria are found in the blood of the diseased animals*; they can only be obtained from the inflammatory exudations by artificial cultivation. Besides, bacteria have *occasionally* been found in the blood of diphtheritic patients, and also in the vesicles of vaccina and variola. The first and last of these diseases are confined to our domestic animals, though splenic fever is also communicable to man.

In examining these diseases separately, we shall begin with *splenic fever* (malignant pustule, anthrax), the disease in which bacteria were first discovered in the blood of the affected individuals. It is chiefly met with in the sheep, cattle, hogs and horses, but may also be communicated by inoculation to smaller animals, as the rabbit, guinea-pig, mouse, etc., and even to man. It has frequently appeared in the form of an epidemic, spreading over certain districts, but is mostly confined to certain rural *localities*, even particular *pastures* or *stables*. The animals affected with the disease usually die one, two, or three days after the appearance of the first symptoms. One particular form of the disease, called the "apoplectic," is, however, observed, which proves fatal a few hours after its appearance. Death is accompanied by dyspnœa and cyanosis; and the chief anatomical changes revealed by the autopsy are enlargement of the spleen, a laky, viscid, dark-colored blood, numerous ecchymoses in different organs, especially in the heart, hæmorrhagic infiltrations into the areolar tissues,

and also hæmorrhages into the alimentary canal. The particular phenomenon, however, now looked upon as pathognomonic of the disease, and bearing directly upon our subject, are the *bacteria* (bacillus anthracis) almost invariably found in the blood of the affected animals, in which they were first discovered and described by *Pollender*, more than twenty-five years ago, and the discovery corroborated soon after by *Brauell*. But it was not until several years afterward that the subject was taken up, and more closely investigated by *Davaine*, *Colin*, *Pasteur*, and, of late years, by very numerous other observers.

The statements of *Davaine*, that these bacteria represented the primary cause of splenic fever, and which first attracted the general attention to this subject, met for some years, however, with considerable opposition. Thus, when in 1869 the French Government appointed a commission, consisting of *Bouley*, *Teilhard*, *Lethérisse*, *Maret*, *Tournadre*, *Bonnet*, *Baillot*, *Chauveau*, *Richard*, *Felgères*, *Messonier* and *Sansom*, to investigate the cause of this disease, *Bouley*, the chairman, reported as follows: 1. That the disease will be produced by inoculation, even with blood containing no bacteria; 2. That the anthrax blood, containing bacteria, loses its violent properties by dessication, without regaining them by the addition of water, though the bacteria may still be present; 3. The blood of rabbits dying from splenic fever always contained bacteria, whether the disease had been produced by inoculation with blood containing bacteria, or with blood containing some of these organisms. Finally, experiments made by inoculating calves and sheep, showed that bacteria were not always present in the blood, and that the blood proved equally virulent, whether containing bacteria or not.

Now although in the majority of cases of splenic fever bacteria are met with in the blood and tissues of the animals, there have been, nevertheless, a number of cases observed in which they could not be found, and others in which they could only be discovered shortly before or after death, a circumstance which gave rise to many doubts as to whether these organisms were really the primary cause of the disease, or whether they were to be regarded as epiphenomena. These exceptional cases, however, were explained, only a few years ago, by *Koch*, who, by artificially

cultivating the bacteria in the anthrax-blood, discovered that the rod-like, motionless bacterium lengthened into a filament, in which finally a number of very minute bright spores appear. These, when liberated by a disintegration of the filament, develop into other rods, and when introduced into the blood of an animal, rapidly increase by transverse fission. From these observations it was supposed that in those fatal cases of splenic fever in which, in the beginning of the disease, no bacteria had been met with, the germs only had been present, to develop into rods shortly before death.

Notwithstanding this discovery, the question was still disputed, and gave rise to the most animated discussions in the medical-scientific world, and to very numerous investigations upon the subject, with hundreds of experiments upon various kinds of animals. Thus facts were brought forward from one side, to be disproved and contradicted from the other. But in carefully studying the results of the numerous investigations, the impartial observer must confess that in splenic fever, at least, it *appears* that the noxious influence upon the organism must be attributed to the presence of these bacteria. Perhaps the strongest proof of this assertion was forwarded, several years ago, by Davaine, consisting in the fact, afterward corroborated by others, that in pregnant animals the blood of the mother, containing bacteria, will, if introduced into the blood of a second but healthy animal, give rise to the same disease, and moreover cause certain death; while the blood of the foetus is free from bacteria, and unable to reproduce the disease in another animal or to cause death. The placenta, therefore, performs in this case the part of a natural filter, allowing the liquor sanguinis to pass through it and enter the circulation of the foetus, while the bacteria with the blood corpuscles are kept behind in the blood vessels of the mother.

Strong as this fact appears to be in favor of the correctness of the bacteria theory relating to splenic fever, it has not been sufficient to dispel all doubts on the question, which will probably not be satisfactorily solved until the origin of these particular bacteria is positively known,—that is, until it has been decided whether the affected animals receive them from the air they breathe, or from the water they drink, or whether they are contained in the

ground of the infected pasture ; and, lastly, whether they may not spontaneously be formed from organic granules, set free by the disintegration of some of the tissues within the animal itself, representing the process of "heterogenesis," as proposed by *Bastian*.

As regards their presence in the air, I have not met with any statement relating to it in the literature of splenic fever within my reach ; though, regarding this matter, *Bender* suggested, a number of years ago, that these organisms might be contained in the green slime frequently covering not only the wooden troughs, but also the buckets and other wooden vessels containing the drinking-water of animals. *Bollinger*, who closely studied the epizootic of splenic fever, prevailing some years ago among the domestic animals of the Bavarian Alps, states that in the Alps, as in all other places where splenic fever prevails, the condition of the ground plays a prominent part, inasmuch as the swampy and moist terrain of the Alpine pastures, like that of the valleys, favors the conservation and reproduction of the infectious poison, and that splenic fever never arises primarily from the ground, but is only produced when the latter has been previously impregnated with the poison, and that the so-called miasmatic origin of splenic fever cannot be proved. He thinks that the dung of the diseased animals plays a more important part than is generally imagined.

Davaine made some experiments for the purpose of showing that the splenic fever poison was transmitted from one animal to the other, and even to man, by the proboscis of flies ; his statements, however, have been contradicted as well as confirmed by other observers.

Acknowledging the probability that in splenic fever the bacteria, themselves, represent the infectious poison, I forbear citing the numerous experiments, made by Pasteur and others, for the purpose of deciding the question, and the interesting discussions to which they gave rise, but dismiss the subject by reminding the reader of the fact, that these bacteria differ in some respects from those which have been met with in the other diseases mentioned ; though *Ewart*,* one of the later investigators of the bacillus an-

* Quarterly Journal of Microscop. Science, April, 1878, p. 168.

thraxis, states that micrococcus-forms, bacterium forms, bacillus-forms, and spore-bearing hyphæ are in nowise generally distinct, but that they are simply phases of the same life-history,—a life history doubtless common to all other bacteria.

The next disease to be considered is the *Typhus recurrens*, or so-called “relapsing fever,” a contagious and epidemic disease, principally occurring in Germany, Russia and Ireland, but also in India; it is accompanied by affections of the spleen, liver, intestinal glands and kidneys, and by hæmorrhages in different organs. In the fever the pyrexia, lasting from two to six days, is followed by an apyrexia of eight or nine days; then follows a second paroxysm or relapse, lasting four or five days, and terminating in a profuse perspiration. Some cases terminate after the second paroxysm, though in many others other paroxysms are observed to occur. But the characteristic phenomenon of this disease consists in the appearance of minute filamentary organisms in the blood of the patient, first discovered several years ago by *Dr. O. Obermaier*, of Berlin. These organisms, called *spirochaete*, or *spirilla*, represent exceedingly fine and delicate filaments, from $\frac{1}{100}$ to $\frac{5}{100}$ mm. or more in length, appearing and moving very rapidly in the form of a close spiral, similar to a cork-screw. They are only observed in the blood shortly before and during the period of pyrexia, while during the apyrexia or intermission, they are entirely absent; neither can they be discovered in any of the secretions of the body. The singular relation existing between the appearance of these spirilla in the blood and the paroxysm of the fever, naturally suggests the idea that they probably are the true cause of the latter; and the explanation of the appearance and disappearance of these organisms in the blood of relapsing fever patients is, that while the spirilla die at the end of each paroxysm of fever, they leave their spores behind, from which during the apyrexial period, a new brood arises; these spores, however, have *as yet not been discovered*. As regards the transmission of the disease from one individual to the other by inoculation with the blood, the statements are contradictory, though it appears that it can be done, provided the blood is taken from the patient during the period of pyrexia. At the same time, however, the same results were obtained by inoculation

with blood containing *no* spirilla, and taken from cases not differing from the others in severity.

In the July number, 1879, of the *Quarterly Journal of Microscopical Science*, an article, entitled, "The *Microphytes* which have been found in the *Blood*, and their *Relation to Disease*," by *Timothy Richards Lewis*, M.D., Surgeon, Army Medical Department, Fellow of the Calcutta University, was published, which originally formed part of the Memoir on the Microzoa and Microphytes of the Blood, appearing as an Appendix to the "Fourteenth Annual Report of the Sanitary Commissioners with the Government of India." This paper, in which Dr. Lewis, who is sufficiently known as an authority on this subject, discusses the whole germ-theory in an able manner, has attracted considerable attention: and since, as regards "relapsing fever," he speaks from personal experience and observation, I shall take the liberty of citing some of his remarks. He puts the question, why we should not at once admit that splenic disease is caused by bacteria rods, and that the aim of treatment should be the destruction of the vitality of these rods; or that recurrent fever is caused by screw-bacteria, and such remedial measures resorted to as tend to destroy them. Answering this question, he says:

"Before such views can serve as the basis of anything like rational treatment, it must be shown—1, either that these organisms, as ordinarily met with, are injurious when introduced into the animal economy; or, 2, that the forms found in disease are in some respects morphologically different from those known to be innocuous—such a difference, at least, as Virchow suggests, as exists between hemlock and parsley."

"With regard to the first point, it has been shown over and over again that all the representatives of the group of fission-fungi can be introduced into the system with the greatest impunity. Not only is their complete innocuousness practically put to the test by every individual at every meal, but observations have been published which have conclusively demonstrated that they may be introduced directly into the blood by injection into the veins, or indirectly, through the lymphatics in the subcutaneous tissue, without the slightest evil consequences. These facts are so well known and generally accepted that it is not necessary to refer to special⁷ observation."

“With regard to the second question, however, diametrically opposite opinions are held—all the advocates of the germ theory with very few exceptions, maintaining that the particular organism, in the particular disease in which they are specially interested, is wholly distinct from all others; that is. if the organism happens to be anything more definite than a granule or molecule. The diseases which have been specially cited as being associated with microphytes may be divided, roughly, into two classes, according to the form of the attendant microphyte—the septinous group, consisting of malignant pustule, septicæmia, and the malignant erysipelas or “typhoid” of the pig, on the one hand, and a low form of fever, commonly known as typhus recurrent, bilious remittent, etc., on the other.”

“With reference to the organisms which have been found associated with the first-named group, taking malignant pustule as the type, it is to be observed that Mr. Robin in 1865 pronounced the bacteridia of Davaine to be identical with *Leptothrix buccalis*; and the well-known botanist Hoffman has stated his opinion that they do not differ from like bodies which appear in milk and in meat solutions. Ferdinand Cohn, again, in his observations as to the growth of bodies of the same character in hay solutions, declares that the bacilli in the latter are identical in form and size with those found in splenic disease, and that the various stages in their development correspond in every particular—the only difference which distinguishes them being that, whereas *bacillus anthracis* presented no movements, the *bacillus* of hay solutions did. This distinction, as has already been stated, has disappeared.”

“Several years ago Dr. Cunningham and myself were, whilst conducting various observations together, frequently struck with the rapidity with which organisms appeared in the blood and tissues of animals after death in this country. The microphytes were not limited to minute spherical and elongated bacteria, but there were also present well marked staves and filaments. In a report submitted by us in 1872, and again in 1874, we drew attention to this matter and suggested the similarity between them and Davaine’s bacteridia.”

"A short time ago a circumstance occurred which drew my attention in a special manner to these organisms. Mr. Hart, a veterinary surgeon in Calcutta, forwarded to me for examination a little perfectly fresh blood which he had removed from a horse which had died that day of well marked anthracoid disease. His curiosity had been aroused as to the microscopical characters of the blood by perusing an account, in the *Veterinary Journal*, of 'worms' having been found in the blood of horses suffering from a similar affection in the Punjab. A slide was prepared and examined under the microscope at once, but no marked peculiarity could be detected; but when this and other slides were re-examined twelve hours later, having in the meantime been kept under a bell-glass, numerous staves and filaments were observed, which, as to size and form, accurately corresponded with the description of like bodies characterizing the blood in anthracoid disease of Europe."

"Several cultivations were started by adding a little of the blood to fresh aqueous humor. The preparations were then set aside for a few hours in a moist chamber. As the temperature of the atmosphere at that time was generally over 90° F., no special appliances were necessary for supplying artificial heat. The development of the rods into filaments and subsequent appearance of highly refracting oval bodies in the latter, corresponded so completely with what Cohn, Koch, Ewart and others had described, that it is not necessary to give figures of the changes that took place. A series of such cultivations was conducted by transferring a little of the last cultivation to fresh aqueous humor, and so on from one preparation to the other."

"It was then determined to ascertain whether the bacilli found in the blood of animals, which had been set aside for a few hours after death, would manifest, under like conditions, similar changes during their growth. Rats were obtained, killed by means of chloroform, and set aside for from three to twenty-four hours, or longer, according as the temperature of the atmosphere was high or low. The results proved that, almost invariably, bacilli were to be found in their blood, in the spleen and in other organs. On one occasion the rapid appearance of organisms after death was exemplified in a somewhat remarkable manner, and possibly the

mode of death was not without some influence in determining their exceptionally early and plentiful appearance."

"The man employed to procure the rats, determined that he would get a sufficient number to last for some time, and proceeded to a large granary with his rat-traps. Having, however, found that he could procure more than could be accommodated in the cage which he had brought with him, he obtained a large earthen vessel, transferred twenty-seven rats into it, and tied a piece of cloth over the mouth of the vessel. As may be supposed, the rats had perished before he got home—all except one."

"I examined the blood and the spleen of twenty of these rats, within about six or eight hours after their having been caught, and found in each case that there were innumerable bacilli present, in every way morphologically identical with *bacillus anthracis*. In some of the cases the number was astonishing. They were present chiefly in the form of rods, but here and there some were seen to have grown to such a length as to cover two fields of the microscope."

"This experience tends to give support to the statement made by M. Signol before the French Academy, to the effect that motionless bacilli, identical with those found in carbon, will be found in sixteen hours or less after death in the blood of animals which have been asphyxiated by means of a charcoal fire. M. Signol, moreover, found that eighty drops of this blood would kill a sheep or goat very rapidly, notwithstanding that putridity could not be detected, so far as appearance and odor went; but that bacilli would not be found in the blood of the inoculated animals, either before or immediately after death."

"It has been urged that the microphytes which appear in the blood after death simply make their way into it from the intestinal canal as a result of the breaking down of the tissues. This objection is certainly no longer tenable, for many observers have shown that if some of the organs be removed from the body immediately after death, or indeed isolated from the circulation whilst the animal is still alive and under the influence of chloroform, these organisms will nevertheless appear if the preparation be kept for some hours at a suitable temperature."

In discussing the probabilities in favor of the bacilli and spirilla of the blood being epiphenomena, Dr. Lewis remarks :

“ There is one circumstance in connection with the microscopic appearance which these organisms sometimes present, which deserves special mention, as it may serve as an explanation for their sudden disappearance from the blood ; and that is, that they may present a well marked beaded or rosary-chain appearance. This feature I was able to observe on one occasion only. The spirilla of the ordinary character were plentiful in this person's blood on the evening previous to the day on which this observation was made, but when examined on the following morning, there were only linked or rosary-chain spirilla in his blood. They were not very numerous, and their movements were not of that *rushing* character ordinarily observed, but conveyed the impression of tumbling across the field.”

“ The inference which such an observation appears to warrant is, that when the blood acquires a certain as yet undetermined condition, it becomes unadapted to the existence of spirilla, and that the fibrils thereupon undergo segmentation, after the manner of other schizomycetes, and the separate plastides become diffused throughout the circulation ; possibly, they then gradually disappear in the same manner as we have seen other plastides (minute bacteria, etc.) disappear after being injected into the circulation. This appears to me the more probable than that they continue in the circulation until the blood re-acquires the state suitable to their growth into fibrils, seeing that the time for their return is so uncertain—it may be two days, may be six days, or a fortnight even, and perhaps they may not return at all. Be that as it may, it is clearly evident that their existence as spirilla is dependent on the composition of the fluids of the body.”

“ Heydenreich suggests that their disappearance is due to the elevated temperature of the blood at the height of the paroxysm. If that were the case, they ought to become more numerous with the fall of the temperature after death, but it is well known that they disappear exceedingly rapidly when life becomes extinct, in this respect offering a marked contrast to other members of the cleft-fungi group—bacteria and bacilli.”

“The fact of their total disappearance immediately after death, probably even before death actually taking place, is very significant, as showing the extremely close relation which exists between them and the blood in *living* tissues, seeing that when the blood is removed from the body, the spirilla will, under favorable conditions, retain their power of locomotion for several hours or days. What these subtle changes of the blood during fever processes may be, chemistry and physiology have not yet revealed; we can therefore only judge of them by the changes of temperature, etc., of the patient; and, in the particular condition under consideration, by the occasional appearance and re-appearance of spirilla, whose presence is manifestly dependent on antecedent changes. That the temperature commences to rise, and that other subjective symptoms are manifested before the appearance of spirilla, testifies to this, for it cannot be that they can exert an influence before they are themselves existent.”

“Dr. Charles Murchinson, at the discussion on the germ-theory of disease at the Pathological Society, put this matter very closely when he said: ‘The fact that in relapsing fever and sheep-pox distinct forms of bacteria have been found, in no way proves any casual relationship between these diseases and the bacteria, and is readily accounted for by the acknowledged fact that the form taken by many minute growths depends not upon the germ, but upon the nature of the medium in which it grows. Indeed, the observations which have been made on the spirilla of relapsing fever are strongly in favor of this view, for they are present in the blood during the first paroxysm, but disappear before the crisis; are absent during the intermission, but return with the relapse of the fever, and again disappear before the crisis. It seems difficult to account for their appearance and annihilation twice over, except on the supposition that the soil was suitable for their development during the febrile process, and unsuitable when the febrile process was complete.’ A like conclusion must be arrived at regarding the bacilli in malignant pustule, septicæmia, and the so-called typhoid fever in the pig, horse and other animals. With regard to the microphytes just named, it may be confidently stated that they are never to be detected in the earlier stages of the disease, but only at a brief period before and after a fatal ter-

mination. To my knowledge they have never been found in the blood of animals which have subsequently recovered; they have always been recognized only as one of the concomitants of impending dissolution."

The next disease in which bacteria have been met with in the blood is *Septicæmia*, representing a depraved condition of the system, and generally observed in connection with grave surgical injuries, such as extensive wounds, etc., or following some operations, a condition characterized by all the symptoms of blood-poisoning. The anatomical changes observed after death are as follows: Early decomposition, skin of a dirty yellowish tint, with numerous livid spots, due to local congestion. Blood of a dark color, fluid, or imperfectly coagulated; small extravasations found in the heart. The lining membrane of the heart and aorta is more or less stained by imbibition of the coloring matter of the blood. The spleen is usually large and soft, very friable and pulpy; the parenchyma of the liver and kidneys is congested or undergoing more or less granular degeneration. In a number of cases, metastatic abscesses are found, especially in the lungs and liver. In surgery, septicæmia is identical with pyæmia, a condition which for a long time was supposed to depend upon the absorption of pus from the surface of the wound into the blood. Though it remains doubtful whether healthy pus could produce this condition, it appears to be generally accepted that the infectious poison enters by the way of the wound, to give rise to a putrid infection of the whole organism, accompanied with or without the formation of metastatic abscesses. But, as in the latter, which are supposed to be formed by embolism, bacteria were found in many cases, this fact was taken hold of by the germ-theorists, and the question arose, whether the septicæmic phenomena were due to the absorption into the blood of a specific septic poison, arising from putrid changes in the pus secreted from the granulating surface of the wound, or even to a putrid decomposition of certain emboli in the blood vessels; or to the noxious influence of *bacteria* contained in the surrounding air and finding their way into the blood through the surface of the wound. The antiseptic dressing of *Lister* is based upon the assumption that the bacteria, settling upon the wound, cause a putrefactive fer-

mentation in its secretion, which, if absorbed into the blood, gives rise to the symptoms characterizing septicæmia.

The idea that septicæmia depended upon the presence of a putrid poison in the blood gave rise to very numerous experiments made upon animals for the purpose of artificially producing this condition by the inoculation or injection of various putrid substances, such as putrefying blood, muscle, urine, etc., containing numerous bacteria, and, as was expected, resulted in the production of the same or similar symptoms and pathological changes in these animals, as characterize septicæmia. While some of these animals died, others remained unaffected. When, however, the blood of these animals, *containing bacteria*, was inoculated into others, graver symptoms, terminating in death, were produced, showing that the putrid poison had increased in violence, but without *being accompanied by bacteria*. Davaine, who extended this successive inoculation upon a very large number of animals, found that the intensity of the poison was increased with each animal through which it passed, and that thus the one trillionth part of a drop would suffice to kill a rabbit. The latter presumption, however, has been refuted by Chassaignac, Onimus, and others. The pathological changes found in animals killed with septicæmic blood are similar to those of septicæmia in man. They are: peritonitis, pleuritis, enlargement of the spleen, pneumonia, hyperæmia of the kidneys, jaundice, and hyperæmia of the intestines. But while bacteria are almost always found shortly before or after death in the blood of animals inoculated with anthracoid blood, it is only an exception to the rule when they are met with in the blood of animals inoculated with septicæmic blood. In fact, all symptoms of septicæmia may be observed, *without the appearance of bacteria in the blood*. The bacteria found in septicæmic blood or abscesses mostly belong to the first group, the sphero-bacteria, and are generally met with in the form of zooglea-patches. In fact, with the exception of splenic and relapsing fever, it is almost exclusively the micrococcus form that has been met with in other diseases, wherever bacteria have been met with in the blood and secretions, or upon suppurating surfaces. And it is especially this fact which the germ-theorists have hitherto failed to bring into harmony with their theory.

As in splenic and typhus recurrent fever, in septicæmia, also, the question has been whether these bacteria, found in a number of cases, are the direct cause of the disease, or mere accidentally accompanying phenomena ; or, whether the symptoms of the disease depend upon the influence of a noxious poison formed within the organism itself. In order to decide this question, various experiments have been made, too numerous to be cited in this place. In these experiments, the most difficult part was to effect a separation of the bacteria from the putrid liquid containing them, and different methods have been followed to accomplish this task, one of which, first resorted to by Pasteur, consisted in filtering the liquid through porous substances, like burnt-clay vessels, by which proceeding the bacteria were left behind in the residue upon the filter. Separate inoculations with the filtered liquid or with the residue, were then made upon different animals, but as the statements of the results of these experiments are so contradictory, I forbear mentioning them ; for while, on the one hand (Pasteur) it is stated that the filtered liquid had lost its virulence, we are told by other observers (Anders, Satherthwaite, Curtis and others) that the septic poison, after the destruction of the bacteria, retains its noxious properties. Even injections of putrid liquids, containing bacteria, into the blood of animals, is not always followed by septicæmic symptoms. Thus *Livon*, who injected such liquids into dogs, observed no traces of parasites in the blood, nor any pathological changes in the organs. Davaine found that septicæmic blood loses its virulence by putrefaction, and, moreover, that the blood taken from *living septicæmic* animals had no deleterious effects upon the animals when introduced into the blood.

The micrococcus form of bacteria, besides being met with in the blood and secretions of pyæmic patients, has in a limited number of cases also been observed in diphtheria, especially in the diphtheritic exudate and ulcers, through which, in some instances, they may have found access to the blood ; furthermore, in the vesicles or blisters of erysipelas, and even, in exceptional cases, upon the valves of the heart in endocarditis. The experiments and observations relating to the presence and signification of bacteria in the diseases mentioned are very numerous, and the

literature of the subject has become too extensive to be thoroughly discussed in this place. Nevertheless, in order to enable the reader to judge for himself, I shall cite the observations and views entertained by some of the leading pathologists. Thus, *Billroth** frequently found cocco-bacteria in the different normal fluids of the body, especially in the serum of the pericardium; but by no means constantly in the blood of the cadaver of persons who had died of septicæmic diseases, even not in the purulent contents of abscesses, etc. The report is as follows:

“The essential cause of their accumulation was most probably here and there the higher or lesser degree of decomposition of the parts. A conditioning relationship of these organisms to the coagulation of the milk, as well as to the alkaline fermentation of the urine cannot be presumed, as they can be demonstrated, here and there, long before the commencement of the process. It is known that the pus of ordinary, apparently healthy granulating wounds contains numerous cocco-bacteria; also, that the quantity of those present stands in no definite proportion to the height of the fever or other general phenomena of the patient. In the reverse, cocco-bacteria are found in the pus of closed abscesses without the co-existence of fever, while, on the other hand, in similar cases, high fever may exist without cocco-bacteria being detected in the pus. Finally, the presence of cocco-bacteria cannot arbitrarily be asserted from the bad odor of the pus. A very similar relation exists in erysipelas; while, namely, the serous contents of the blister in half of the cases examined contained no cocco-bacteria, they were found several times under the epidermis in non-specific exudations.”

“All forms of cocco-bacteria may be produced by artificial cultivation from those parasitic forms found in the organism under different pathological conditions, though none of them would, in its different phases of development, show any essential character differing from those bacteria associated with putrefaction. From this it results that not even a specific mode of appearance is observed on the bacteria met with in the animal organism, opposite to those found in inorganic nature. Still less can this be asserted for definite pathological conditions, or for individual diseases.”

**Virchow u. Hirsch*, Jahresbericht f. d. Jahr 1874. Vol. I, p. 353.

“The conditions essential to the regular growths of the cocco-bacteria are only partially known and certainly very complicated; the presence of decomposable organic substances is by no means sufficient, which is proved by the want of correspondence between the commencement of putrefaction and the appearance of these organisms, and also by a want of a definite proportion of the quantitative extent of the latter to the putrefaction. The presence of water and air, as well as a certain passivity and undisturbed position of those parts serving as a place for their germination and proliferation, are undoubtedly essential for the development of the organisms. But even then, a further increase in numbers does not take place, as the experiments of Max Wolff have proved, who failed to observe any septic phenomena after the introduction of fungous spores into the trachea, though he could detect the particles in question in the circulating blood. The numerous statements concerning the presence of bacteria in the blood in this or that disease, also, must be received with great care, as there are other very pronounced cases of septic disease in which they are, at least in entirely fresh blood, certainly wanting. It appears, therefore, that the ‘vitality’ of the organism exerts a certain conditioning influence upon the destruction, that is, the further growth of the accumulated fungous germs in the circulating blood, as in the secretions and exudations, etc. This fact may be better understood in presuming that the cocco-bacteria are unable to assimilate the albuminous bodies of the tissues in the form in which they exist in the living organism.”

“In order that this assimilation may take place, a third hitherto unknown body, which Billroth designates as ‘phogistic zymoid,’ must take part, that is, a body which, produced in the course of the inflammation and by this process itself, excites the fluids of the tissues, the blood, pus, etc., in the way of a ferment, rendering them a suitable and accessible medium for the multiplication of the dormant germs contained in it. This phlogistic zymoid acts wherever it comes in contact with healthy tissues, but still more rapidly upon diseased tissues, in exciting them to inflammation and causing their destruction. By the disintegration of the tissues thus induced, the ground becomes prepared for the unlimited proliferation of these parasitic vegetations. In

speaking of the putrid infection, Billroth declares it very probable that the respective poison arrives in the organism in the form of a finished chemical body, exhibiting its effects in proportion to its respective quantity: a conception standing in opposition to the generally adopted fermentation theory."

"*Panum*,* in continuation of his former investigations concerning the relations of the parasitic organisms to putrid processes, published in 1856, renews the question whether the microscopical organisms contained in the ordinary putrefying liquids, can be regarded as the intermediate cause of that group of symptoms designated putrid or septic infection. As he was unable to deprive, by long continued and energetic boiling, putrid liquids of their noxious properties, by which the vegetable organisms contained therein must necessarily be killed,—he arrived at that time at the conclusion that their deleterious properties could not be ascribed to the micrococci and bacteria, but to another lifeless agent, representing a pure chemical body. Before passing to his more recent experiments, corroborating this view, he endeavors to demonstrate by deduction the improbability of the doctrine, according to which the bacteria are the direct septically acting agents. He mentions the fact, confirmed by Magendie, Gaspard and Stich, that the intestinal canal of healthy men and animals contains an extractible substance, which injected into the blood gives rise to all phenomena of sepsis. The circumstance that the latter do not appear under normal conditions, has been made use of as a proof for the molecular, insoluble nature of the poison. But bodily substances also, even psorosperm-eggs, may pass through the epithelium of the intestine, while many soluble substances, as a solution of curare, for example, pass with extraordinary difficulty. Against this supposition is, furthermore, the fact that the duration of the time of incubation in septic diseases does not stand in the reverse proportion to the quantity of the injected liquid, which certainly should be the case. As regards, finally, the clinical symptoms of the septic infection, Panum finds the characteristic points of the course not in the curve of temperature, but in the affection of the intestine, announced by vomiting and

* L. c. p. 354.

diarrhœa ; then in the severe collapse and depression of the entire nervous system, and finally in the rapid appearance of putrefaction and the imperfectly coagulating blood. From this it is clear that we are not justified in forming from the symptoms of the poison, and especially from the duration of the course of the affection depending upon the dose and the concentration of the injected liquid, an argument for the essential participation of the at the same time introduced microscopical organisms."

"Against the bacteria-theory, the fact that a putrefying liquid, rendered perfectly free and clear from bacteria by repeated and continued filtration, loses nothing of its septic effects, may be most strikingly brought forward. Of the entire absence of bacteria, Panum became convinced by means of high amplifications. By a repetition of the experiments with putrid liquids, which had been boiled for several hours, he became still more convinced that the distillate obtained from it, though of a bad odor, was not poisonous, while the deleterious effect of the residue was little less than that of the original liquid. The alcoholic extract of the clear filtrate of meat, which had been macerated for four weeks, injected into the blood of a dog, shows only a hypnotic effect. The residue, not extracted by alcohol, however, gives rise to vomiting, inclination to stool, tenesmus, and general depression, from which the animals scarcely recover after the lapse of twenty-four hours. In view of such results, Panum regards it as settled that the putrid poison is no simple extractive substance, but a real chemical body; the latter being the true deleterious agent in the septic processes, while the part of the bacteria is only accidental."

"As regards the origin of this putrid poison, Panum suspects that it is produced by the vital process of the bacteria, perhaps a kind of secretory product (Bergmann). All experience relating to the presence of bacteria in the healthy organism of man, as well as in the diseased, teach us at least that, both bacteria and processes of putrefaction, stand in near relationship to each other. Though, undoubtedly, bacteria also appear in the body with decomposition, their increase always depends upon the cessation of life, be it from local or general causes."

"*Hiller*,* in concert with the results of his former researches,

* *L. c.*, p. 365.

states that in putrefying urine the degree of development of bacteria does not always correspond with the height of the decomposition, but that both rather stand to each other in a reversed proportion. Thus, urine left in an open, or corked, perfectly clean bottle, does not, even after two months, undergo putrefaction; the latter, however, took place in some bottles closed with cotton, and even earlier. Notwithstanding this difference in the results of the experiments, all specimens of urine corresponded in containing a rich vegetation of bacteria after the lapse of only a few days. But this vegetation only attained a certain low degree, and then remained stationary, if afterward a putrid decomposition was not induced. As long as the urine standing open was acid, mycelii of *Penicillium glaucum* and *Torula cerivisiæ* would grow, but with the commencement of alkaline reaction, the former disappeared entirely, and the latter partially. As regards the influence exerted by various chemical agents upon the putrefaction, ammonia and carbonate of ammonia remained negative. Phosphate of potassa and soda, as well as tartrate of ammonia, promoted in a high degree the development of bacteria, without producing putrefaction. Specimens of fresh urine placed in a close vicinity to putrefying urine showed extensive masses of bacteria without putrefaction."

"The putrefaction of urine, therefore, cannot be looked upon as a physiological phenomenon of exchange of matter through the bacteria. The vegetation of the latter depends solely upon the supply of assimilable nutritive matter, which, being scanty in fresh urine, is only furnished in sufficient quantity through the putrefaction. As, now, the bacteria are powerless to split the complicated (nitrogenous) combinations in the urine, it requires, in order to nourish them, an agent that induces the decomposition. This agent becomes active through and with the putrefaction, and from this the coincidence, regarding time and extent, of putrefaction and bacteria may be explained.

"In setting aside the decompositions taking place in the urine, experiments with chicken eggs show moreover that bacteria alone are incapable of inducing a putrefactive decomposition in the albumen. From this Hiller concludes the untenability of the vitalistic theory; he is moreover of opinion that the ferments of

putrefaction are represented by most minute protein-particles, which, being themselves in a state of putrefaction, are carried through the air to the substratum, to involve it in a similar decomposition."

Although it had been stated by Pasteur, and corroborated by other observers, that no microscopic organisms could be detected in the healthy blood, experiments were nevertheless also made to prove the contrary. Thus, *Fiegel* took pieces of different organs of freshly killed animals, and fastening them to a previously boiled silken thread, plunged them successively into boiling paraffine, until they were covered by a thick layer of this material. Though it was supposed that the boiling heat of the paraffine had destroyed every living germ at the surface of the pieces, and that the layer of paraffine covering them had prevented the access of fresh germs, bacteria were nevertheless found in the interior of the pieces after the lapse of several days.

These experiments, while attracting much attention, were repeated by other investigators; and as the results obtained corroborated the statements of *Fiegel*, it was concluded that the bacteria themselves, being always present in the blood, were unable to exert any influence upon the production of pathological processes. *Burdon-Sanderson*, also, investigated this matter, by repeating *Fiegel's* experiments with a slight modification, rather improving the process; and in referring to the experiment in one of his lectures on "The Infective Process of Disease," delivered in the University of London, about two years ago, he says that under the conditions described it seems to him quite impossible to suppose, either that germs could penetrate to the organ (the liver or kidney) from the outside, or that any germ encountered by the organ in its transference from the body of the animal to the basin could escape destruction. If, therefore, bacteria be found, they or their germs must have been there before the organ was plunged into the hot liquid. From these experiments, he then concludes that germs continually enter the blood by the way of the portal circulation, and as no bacteria exist in the circulating blood, the alternatives are that, either the bacteria are formed and immediately afterwards destroyed, or they are not allowed to germinate, because the conditions under which they are placed in the living organism are such as to prevent their development.

Burdon-Sanderson then fully endorses the correctness of Fiegel's statements, and moreover explains the phenomenon according to his own views. But in looking again at the other side of the question, it will be found that *Chiene* and *Ewart* definitely proved the absence of bacteria or any other organism, if removed and examined under a spray of a solution of carbolic acid, together with other precautionary measures, taken during the experiment to prevent the access of these organisms. From these experiments we may learn to receive the statements regarding the presence of bacteria in the living blood or tissues with a great deal of caution.

The last disease in which bacteria have been obtained by artificial cultivation from the secretions of the animals, is *infectious pneumo-enteritis* of the pig, as it has been termed by **Klein*, who investigated it two years ago. The anatomical changes met with in this disease are similar to those already mentioned in connection with splenic fever, consisting principally of lobular pneumonia, ulceration of the mucous membrane of the intestines, the liver, spleen, etc. The results of these experiments showed : 1. That the fresh blood of diseased animals does not, as a rule, contain the virus, as it fails to produce the disease when introduced into a healthy animal. 2. That the fluid as well as the solid lymph of the diseased peritoneum contains the virus in a very active state. 3. That parts of the diseased lung, ulcerated intestines, and diseased spleen contain the virus in a very active state; a frothy, blood-containing matter, found in the trachea and bronchi, also possesses infectious properties, showing that the breath of the diseased animal is charged with poison. 4. That infection is produced by cohabitation with a diseased animal, or by keeping healthy animals in a place whence a diseased animal has been removed.

By some other experiments, Klein furthermore showed that the virus may be cultivated artificially outside the body of an animal, as had been successfully done by Koch, in splenic fever.

The cultivation of the virus consisted in taking a minute portion of the lymph, and inoculating a drop of fresh aqueous humor

* *Quarterly Journ. of Microscop. Science*, April, 1878.

of a rabbit, which, being placed upon a glass-slip and arranged in such a manner as to prevent the access of air, was kept in the incubator for twenty-four hours at a temperature of 33° to 34° C. With a minute portion of this fluid another drop of aqueous humor was inoculated and treated in the same manner; and from the latter a third, representing the third generation. Two animals inoculated with this matter were affected with the disease.

In a similar manner the virus was cultivated from a minute portion of solid lymph of the peritoneum of a diseased animal, and the cultivation carried to a fourth generation. Two animals, having been inoculated with this matter, became smitten with the disease. And, in carrying the cultivation of the virus to the eighth generation, the matter remained still effective.

The microscopical examination of the cultivated liquids showed that they were the seat of the growth and development of a kind of bacterium, resembling *Bacillus subtilis* of Cohn, and representing a fine delicate rod, thinner than the *Bacillus anthracis* of Koch.

The interpretation which Klein gives of the results of his experiments appears to me, for several reasons, not calculated to inspire much confidence. Thus, failing in transferring the disease from one animal to another by the inoculation of the blood, he succeeded with the inoculation of the exuded lymph, showing that the virus really existed in the latter. In this paper he omitted to mention whether he had examined the blood and lymph microscopically, and whether, or not, he had detected bacteria in one or the other; but, as he resorted to artificial cultivation, it may be presumed that he did not. Obtaining at last these organisms with their spores, he looks upon the latter as the true cause of the disease, without explaining whence they came. If they had been the true cause of the disease, they must have originally existed in the blood, from which they might have gotten into the secretions, and Klein should have detected them in this fluid as readily as he discerned them in the cultivated liquid. Besides this, he says: "After many failures—owing to the introduction of *Bacterium termo*—I succeeded at last in obtaining, already in the second generation of original virus, a

pure crop of bacillus and its spores." Now, as will be seen, that with the bacillus he also obtained bacterium termo, rendering it very probable that both were derived from the same quarter, that is, from the outside; or, if not, that both, according to Ewart, represented different phases of one and the same organism, and, perhaps, originated in the same manner, as the bacilli in the dead mice and rats of Dr. Timothy Lewis. At any rate Klein did not consider it *impossible* for other organisms to make their appearance in the cultivated liquid, as further on he remarks that "no other organisms appeared in these cultivations," *i. e.*, after the bacillus had displaced the bacterium termo. As regards pneumo-enteritis of the pig, therefore, it is more than probable that the peritoneal exudate itself represents the infectious poison.

In comparing the four diseases above described with each other, we cannot but notice the similarity of their accompanying symptoms, and of the anatomical changes observed after death. They are all characterized by an affection of the spleen, lungs, heart and intestines, accompanied by hæmorrhages, ecchymoses, and metastatic abscesses, symptoms depending more or less upon a depraved condition of the blood, and resembling those of typhus fever. Therefore, it may be presumed that the presence of living organisms, found in the blood of the affected men and animals during the last stage of these diseases, is due to the diseased condition of this fluid, affording a favorable medium for their development, and that the diseases themselves are due to the introduction of some specific poison into the blood, representing a product of the diseased organism itself.

That organized bodies may exist in the blood of certain animals without interfering with the normal functions of this fluid is sufficiently proved by the discovery of E. Ray Lankester, in 1871, of a large infusorium in the blood of the common frog of Europe, *Rana esculanta*, which he called *Undulina ranarum*; and, furthermore, by my own, made almost at the same time, of a similar infusorium in the blood of our tree-frog during three successive years. Though these infusoria were quite numerous in the blood of these animals, nothing abnormal could be discovered in the blood of the latter, a considerable number of which I kept under observations for many months.

In addition to the experiments and opinions of a number of distinguished pathologists, which, in the preceding pages, I have brought before the reader, I shall, before dismissing the subject, cite some portions of a remarkable address, delivered, in 1875, to the Pathological Society of London, by Prof. H. Charlton Bastian,* the well known advocate of the theory of "Spontaneous Generation," whose persevering studies into the nature of minute living organisms have not been surpassed by any other investigator. In the course of this address he says:

"I will, however, now briefly enumerate the evidence which seems to me quite sufficient to disprove the probability of the existence of any casual relationship between the lower organisms and the diseases cited at the head of this section, and to establish, on the other hand, the position that the bacteria met with in diseased fluids and tissues are for the most part actual pathological products—that they are, in fact, engendered within the body, or are descendants of organisms owning such an origin, rather than of previously existing organisms introduced from without. It would take too long were I to enter upon a consideration of this evidence. I must therefore content myself with briefly summarizing the principal facts and arguments on which a judgment may be founded."

1. "The experiments of many investigators prove that the alleged causes of diseases may be actually introduced into the blood-vessels of lower animals by thousands without producing any deleterious effects in a large proportion of the cases."

2. "Bacteria, if not actually to be found within the blood-vessels of healthy persons, do nevertheless habitually exist in so many parts of the body in every human being, and in so many of the lower animals, as to make it almost inconceivable that these organisms can be the cause of disease. In support of this statement I have only to say, that even in healthy persons they may be found in myriads in and about the epithelium of the whole alimentary tract from mouth to anus; they exist throughout the air-passages, and may be found in mucus coming from

* The Microscopic Germ-Theory of Disease; being a Discussion of the Relation of Bacteria and Allied Organisms to Virulent Inflammations and Specific Contagious Fevers.—*Monthly Microscop. Journal*, August, 1875.

the nasal cavities, as well as in that from minute bronchi. They exist abundantly amongst the epithelial débris within the ducts of the skin, not only in the face, but in the other parts of the body. Fresh legions of them are also being introduced into the alimentary canal with almost every meal that is taken, whence they may, perhaps, readily find their way into the mesenteric glands, if not farther within the system. And, lastly, in persons with open wounds, bacteria are constantly to be found in contact with such surfaces, especially if the wounds are not well cared for, though the injured person does not necessarily suffer at all in general health."

3. "It is no answer to these difficulties to say that there are distinct species among the lower organisms, some of which are harmless, though others are poisonous (or so-called germs of disease). In support of such an opinion nothing can be alleged save some of the facts whose cause is doubtful; whilst again, such an interpretation may be brought the experiments of several investigators, showing that bacteria are the creatures of circumstance, and modifiable to an extraordinary degree. The last position is even admitted by Professors Sanderson and Lister. The former acknowledges that they are 'the lowest organisms,' and that they are 'much more under the influence of the conditions under which they originate and are developed, than organisms of any other class;' while Professor Lister's own work has compelled him to make an admission which, in the face of facts previously stated concerning the wide distribution of bacteria within the body, seems fatal to a consistent belief in the germ-theory of disease. He says: 'If the same bacterium may, as a result of varied circumstances, produce in one and the same medium fermentative changes differing so widely from each other, as the formation of lactic acid and that of black pigment in milk, it becomes readily conceivable that the same organism which, under ordinary circumstances may be comparatively harmless, may at other times generate products poisonous to the human economy.'"

4. "The considerations now to be mentioned suffice, in my opinion, to complete the discomfiture of the germ-theory as an explanation of the mode of causation of the disease with which we are at present concerned. It is this. It has been shown, on

the one hand, that the virulence of certain contagious mixtures diminishes in direct proportion to the increase of bacteria therein ; and on the other hand, it has been equally proved that fresh and actively contagious menstrua lose scarcely any of their contagious or poisonous properties after they have been subjected for a few minutes, when in the moist state, to a temperature which no living units can be shown to survive (212° F.), or after they have been exposed to the influence of boiling alcohol, which is well known to be equally destructive to all recognized forms of living matter. Such facts have been substantiated by Messrs. Lewis and Cunningham, Sanders and others."

"Having said this much in opposition to the germ-theory, let me as briefly enumerate the facts and arguments which seem to me to show the real relations of bacteria and their allies to the diseases in question. I turn therefore to the construction of an opposite doctrine."

"Admitting, in part, the very frequent presence of bacteria in diseased fluids and tissues, I consider that their presence and import should be differently explained. I say I admit the association in part, though I by no means admit it to the extent alleged. Bacteria are not, for instance, to be found in the blood of persons suffering from pyæmia, as might be inferred from former statements of Dr. Sanderson, which I have already quoted. My own experience in this matter seems to be entirely in accordance with that of Professors Billroth and Stricker. Neither do I believe that the presence of bacteria in inflammatory fluids has the significance which Dr. Sanderson attaches to it, since it has been ascertained by myself and others that the exudation fluid of sick persons suffering from disease of a totally different type are often similarly crowded with these lowest organisms, whilst the recent observations of M. Bergeron seem to show that they may be found even in freshly extracted pus from ordinary abscesses occurring in elderly persons."

"Now, it would seem quite obvious that the consistent advocate of a germ-theory of a disease can only successfully maintain such a doctrine if he can show, amongst other things, that bacteria are more capable of altering the character and chemical constitution of fluids of the body than they are themselves prone to

be altered by independently instated changes taking place in such fluids. It seems, therefore, like unintentionally cutting himself free from the theory to which he has hitherto adhered, when we find Professor Lister, in speaking of the assumed 'special virus of hospital gangrene,' going on to say that 'it is not essential to assume the existence of a special virus at all, but that organisms common to all the sores in the ward may, for aught we know, assume specific properties in the discharges long putrefying under the dressings.' This passage has a similar import to that of a quotation previously made. In both a first plea is assigned to the modifying influence of altered fluids; and however much the correctness of such supposition would tell in favor of cleanliness, free exposure, or even of antiseptic dressings, it is none the less inimical to a consistent holding of the theory on which Prof. Lister has chosen to base his system of treatment."

"But though such statements are adverse to the holding of a germ-theory in the only form in which it may be at all tenable, they are entirely in accordance with my own observations and views. I maintain, in short, that even the very existence of organisms in the fluids and tissues of diseased persons is for the most part referable to the fact that certain changes have previously taken place (by deviation from healthy nutrition) in the constitution and vitality of such fluids and tissues, and that bacteria and allied organisms have appeared therein as pathological products—either by heterogenesis, or by what I have termed archebiosis, or birth direct from a fluid."

"The evidence on which my belief is founded is of this nature :

1. Bacteria and their allies are found in greatest abundance during the life of the individual in connection with dying tissue-elements, and apparently are as plentiful with dying epithelium of the cutaneous ducts, as in parts like the mouth, which are most liable to contamination with organisms from without. Again, they exist abundantly in and about the dying cells of bronchial mucus, although living bacteria appear to be almost completely absent from ordinary air.

2. The microscopical examination of such epithelial or mucous elements also favors the notion that the contained bacteria are products engendered within such cells rather than mere results of

an external contamination and imbibition. This opinion is based upon the following considerations. Bacteria only appear within the cell when it is obviously dying; and in the case of the epithelium, for instance, they manifest themselves at first as minute motionless particles scattered through the semi-solid substance of the cell, where each particle grows into a distinct bacterium, which still remains motionless, and does not appear to divide for a long time. This is precisely similar to what I have observed over and over again, when amœbæ in vegetable infusions get into an unhealthy condition and become resolved into nests of bacteria. They may exist for days in a state of activity with bacteria in the fluid around them, though none are to be seen in their interior. After a time, however, the chemical constitution of the fluid seems to become no longer suited to the amœbæ; their activity ceases, they remain as almost motionless balls of jelly, and soon multitudes of the minutest particles appear throughout their substance, each of which straightway grows into a bacterium. The former amœba is converted into a mere bag of bacteria, which after a time ruptures, and thus liberates its swarming colony of newly-engendered living units. Multitudes of mucous corpuscles seem to undergo the same kind of change, so that bacterial degeneration takes place in the same manner and is almost as typical amongst them as is fatty degeneration amongst pus-corpuscles. The two kinds of degeneration, moreover, commonly occur side by side in epithelial dèbris. Bacterial degeneration takes place where the vitality of the unity is lowered, but where it is not sufficiently degraded to permit of the still lower and more obviously distinctive process of fatty degeneration; and if any one wishes to see it in perfection let him examine some central portion of the kidney or other internal organ of a warm-blooded animal five days or more after its death."

" 3. Bacteria are admitted by nearly all pathologists to be absent from the blood of healthy persons during life, and yet in from eight hours to four or five days after death, according to the temperature of the air at the time, the previously germless blood of individuals may be found to be swarming with these organisms in every stage of growth."

“ 4. Whereas blister fluid or serum has been shown to be free from organisms in healthy persons, I have ascertained that, given a febrile patient with a temperature of 102° F., one can determine the presence of bacteria, as will, in any blister-bleb which remains intact for forty-eight hours or more, and this, too, when the patient does not suffer from any specific fever, but merely from pneumonic inflammation. I was led to ascertain this fact by finding, about eighteen months ago, myriads of bacteria in all the blebs of a patient suffering from acute pemphigus, with a temperature of 103°.”

“ 5. Lastly, as Dr. Sanderson has shown, a chemical irritant, such as liquor ammoniæ, may be introduced beneath the skin of some of the lower animals in such a way as to ‘preclude the possibility of external contamination,’ and yet here, amidst tissues which he has shown to be germless, we may thus, within twenty-four hours, determine the presence of swarms of germs and organisms in the pathological fluids effused under the influence of the local chemical irritant.”

“ This constitutes, as it appears to me, an exceedingly strong body of evidence tending to show that bacteria are pathological products capable of being engendered within the body after death.”

The untenability of the germ-theory has been so ably demonstrated by Prof. Bastian in his address as to require no further comment. In explanation of the origin of bacteria in those diseases in which they have been met with in the tissues, he offers his own theory of “heterogenesis” and “archebiosis.” In examining and considering this subject more thoroughly and without prejudice it becomes difficult to perceive any impossibility in bacteria arising, at least, by heterogenesis, as Bastian designates that process by which the particles of living tissues, on the eve of the death of the latter, may individualize themselves and continue to live in the form of a low organism. For my own part, I venture to endorse what he says.* “Just as the life of one of the cells of a higher organism may continue for some time after the death of the organism itself, so, in accordance with this latter

* Bastian—*The Modes of Origin of Lowest Organisms*. 1871, p. 15.

view, may one of the particles of such a cell be supposed to continue to live after even cell-life is impossible." That cells do live many hours after the death of the animal or man is corroborated by my own observation which I made in 1867—when examining the epithelium of the mucous membrane of the frontal sinuses of a yellow fever patient eleven hours after death—and consisting in the active motion of the cilia of a single epithelial cell, causing a movement of the cell itself, similar to that of an infusorium. The same phenomenon has been observed on ciliated epithelial cells, even at a longer time after death, by other observers. As regards Bastian's theory of archebiosis, it is more difficult to accept it at present, though the fact that there must have been a time in the history of the earth when organic matter arose from inorganic, and in its turn gave rise to the first living organisms, cannot be overcome. The theory of the development of bacteria from putrefying starch granules has been advanced previously to Bastian by *Hartig*, who has even repeated his former assertions. The origin of the minute moving granules found in the vesicles of variola and vaccina, which *Chauveau* regarded as the carriers of the specific poison, also, would find an easy and reasonable explanation by the heterogenetic theory. We may furthermore refer to the observations of Lewis on dead mice and rats, cited already in the preceding pages; and lastly to some of my own, as follows: In 1867, in examining the brains of yellow fever cases, I put small pieces of the cortex cerebri in small wide-mouthed bottles containing a solution of one-third of alcohol to two-thirds of water. The bottles were filled up to the top, so that when the cork was inserted the liquid would run over, and no air be left within. In examining these pieces four days afterward, I found great numbers of minute organisms in their interior. Some of them represented micrococcus, even *zoogleæ*, and vibrios; but there were others, rod-like forms with highly refractive granules, unknown to me at that time, but which I found afterward explained by the labors of Koch and Ewart. In these cases, however, I shall not preclude the possibility of some of these organisms having adhered to the surface of the brain before they were put into the alcoholic solution.

[TO BE CONTINUED.]

Domestic Correspondence.

ARTICLE VIII.

EDITORS MEDICAL JOURNAL AND EXAMINER: Seeing so many editorials on maternal impressions, brings to mind a case which strongly impressed me with the positiveness of maternal impressions.

I was called on May 23, 1881, to attend a multipara, she being healthy in every respect, but that she had burned her wrists some two months before, and that an old lady had told her that her baby would be marked just as she had been burned, that she had better have a doctor, and then, when the baby was born, the doctor could remove the marks, obliterating all traces of them. Therefore I was called. She was crying when I arrived—afraid her baby would be marked. I told her there was no likelihood of that, and to quiet herself, and not cross the bridge until she came to it. But, lo, when the child was born about two hours after, there was the exact counterpart of the burns on the mother. Although the mother's had entirely healed, the infant's looked like a burn done fifteen or twenty minutes afterward. I treated it just as a fresh burn, and it made a complete recovery without any marks remaining.

Does any one need any stronger evidence of the effect of maternal impression?

S. O. STOCKSLAGER, M.D.

BOONE, IOWA, July 21, 1881.

Society Reports.

ARTICLE IX.

MICHIGAN STATE BOARD OF HEALTH.

The regular meeting of this Board was held at Lansing, July 12, all the members being present, as follows: Hon. LeRoy Parker, of Flint; Rev. D. C. Jacokes, of Pontiac; Henry F. Lyster, M.D., of Detroit; J. H. Kellogg, M.D., of Battle Creek; Arthur Hazlewood, M.D., of Grand Rapids; John Avery, M.D., of Greenville; and Henry B. Baker, M.D., Secretary.

Hon. LeRoy Parker was elected President of the Board for the ensuing two years.

SMALL-POX.

Dr. Jacokes spoke of an immigrant tramp-burglar who came down with the small-pox while confined in the jail at Pontiac. He and another prisoner in the jail were removed to the temporary hospital. The prisoner stole the clothes of the immigrant, and, leaving his own, ran away. Some one then stole the prisoner's cast-off clothes and bedding after supposed disinfection, and by this means small-pox was communicated to more than sixteen persons. He also reported that a second immigrant brought small-pox near Pontiac, but the disease was restricted.

Dr. Kellogg reported that an immigrant, sick with small-pox, had recently been put off a M. C. R. R. train at Battle Creek. He remained about the depot all day before it was discovered that he had small-pox. He was then removed to a tent-hospital.

Dr. Henry reported an outbreak of small-pox, apparently brought by an immigrant Dane, who was vaccinated and not sick himself, to a camp in Montcalm county. The immigrant slept with and gave the disease to a countryman who was vaccinated

before his arrival in this country, six years ago. The disease was light, the man not being confined to his bed at all, and finally he went to a family of five unvaccinated persons, all of whom had the disease lightly. There were other cases in the neighborhood coming from the same source. The immigrant probably brought the contagium in his clothing from some infected city or immigrant on the journey, as he said there was no small-pox on board the ship on which he came, though there were cases of diphtheria on board.

A communication was received from the American Public Health Association, asking the influence of this Board to secure legislation making it a criminal offense for any person to communicate any communicable disease, such as small pox, scarlet fever, or venereal diseases, and giving to boards of health and health officials the same power in the prevention and suppression of other diseases as they now possess in cases of small-pox.

The Secretary presented a resolution of the American Public Health Association, asking the Michigan Board to use its influence to secure general vaccination.

THE CHICAGO SANITARY CONFERENCE.

By direction of the Board, the Secretary had attended the conference of delegates from local and State boards of health, held at Chicago, June 29, for the purpose of devising means for preventing the introduction of small-pox and other diseases by immigrants. As Secretary of the Conference, he had prepared an official report and sent it to the National Board of Health for publication in its bulletin.

The action of the Sanitary Conference, to prevent the spread of small-pox, was indorsed, and resolutions were adopted requesting the National Board of Health to secure, if possible, the vaccination of immigrants before they land in this country; asking the attention of every local board of health in Michigan to the details of the plan adopted at the late Chicago conference; calling upon them to secure a careful inspection of all immigrants entering and remaining within their jurisdiction, and a prompt vaccination or re-vaccination with pure and fresh bovine virus of all persons not protected against small-pox; calling attention to the need of establishing a quarantine at Port Huron; also, ask-

ing the National Board of Health to aid in preventing the introduction of small-pox and other communicable diseases by immigrants landing at eastern ports.

Dr. Baker read resolutions of the Tennessee State Board of Health, indorsing the action of the Memphis Board of Health, commending the inspection service of the National Board of Health as being very much more effective than local quarantines, with less detention and annoyance to commerce.

VACCINATION FOR VARIOUS DISEASES.

Dr. Lyster, Committee on Epidemics and other Diseases, read a translation of two important papers recently published in France, on the causation of certain communicable diseases, which gave details of successful methods of making viruses which can be used in vaccination, and are effective in preventing deaths from these diseases. He received the thanks of the board, and was requested to embody his remarks and so much of the translation as was essential, in a paper for publication.

Dr. Baker had paid some attention to the same subject in connection with diseases of animals, affecting the public health. He mentioned a paper by Prof. Law, of Cornell University, suggesting that these protective viruses all seemed to be made in accordance with a general law, namely, by their cultivation in fluids with access of free oxygen, and this gives us great hope of soon being able to make protective vaccination for many of the most dangerous diseases in animals and mankind.

Dr. Baker reported the investigation of an outbreak of a new disease in England, traced to the eating of American hams. The cause of the disease proved to be a virus which was used to inoculate animals of various kinds and reproduced the same disease in them. From the accounts it seems probable that it is no more nor less than our hog cholera. The symptoms closely resemble in some respects the disease known last winter in this country as "winter cholera."

SANITARY ASSOCIATIONS.

Dr. Jakes referred to the Pontiac Sanitary Association, and the work it was doing for public health in that city.

Dr. Kellogg reported the formation of a sanitary association

at Battle Creek, as a fruit of the recent sanitary convention held there by this board. Among the subjects brought before the association was that of impure water. He had examined a sample of water used at an eating-house, among the boarders of which there were seven cases of typhoid fever last year. It contained a large amount of organic matter. Also a sample containing organic matter and a large amount of chloride of sodium, used by a family in which there had been much illness.

The request of the sanitary convention at Battle Creek, that this board issue a circular on criminal abortion, was referred to Dr. Kellogg as special committee.

FUTURE PUBLICATIONS.

Drs. Lyster and Baker reported their revision of the document on the Restriction and Prevention of Diphtheria, and different points were discussed, amended and the document adopted. Thirty thousand copies will be printed and the document stereotyped, so that local boards of health may secure any number of copies at cost of paper and presswork.

Dr. Jacokes referred to the great lack of knowledge among those who ought to know, as to what constitutes thorough disinfection. He proposed to remedy this by the preparation of a circular on disinfection.

Drs. Baker and Kellogg were appointed a special committee to prepare a tract on disinfection, which shall give the best method adapted to each disease and to each article to be disinfected, and which shall call attention to the many useless substances now employed for such purposes.

The document heretofore issued on the treatment of the drowned being out of print, it was referred to a committee of revision, with a view to its republication.

Dr. Baker was instructed to prepare a paper on the best-methods of constructing hospitals for communicable diseases, avoiding the use of the name "pest-house."

LEGISLATION UPON HEALTH.

Hon. Leroy Parker, Committee on Legislation, made a report relative to public health acts passed by the last legislature, giving the titles of forty-eight acts bearing directly or indirectly on pub-

lic health subjects, mentioning the subject of each act. These acts give increased powers to local boards of health, additional appropriations to the State Board of Health, authorize the board to control State swamp lands to appropriate lands to drain overflowed lands, etc.

The recommendation by Dr. Foster Pratt, of Kalamazoo, for the amendment of the law relative to reporting dangerous diseases, was referred to the Committee on Legislation.

Under the new appropriation made by the recent legislature, the board authorized the purchase of additional meteorological instruments for the use of the board's observers in different parts of the State

WORK IN THE OFFICE.

The Secretary read a report of work in the office, which included a statement of a number of health officers appointed for the present year, beginning April 1, as follows: In townships, 797; villages, 107; cities, 32; total, 936. For the year ending April 1, there were appointed, for townships, 827; villages, 112; cities, 36; total, 975—a difference of 39. It is believed that more than 39 will be returned before the close of this year. The 975 health officers appointed for 1880 made annual reports to this board, as follows: Of townships, 414; of villages, 55; of cities, 14. Clerks of local boards of health have sent annual reports for 1880: From townships, 501; from villages, 36; from cities 7. The compilation of these annual reports is now in progress.

During the quarter the correspondence of the office has been materially increased. This was partly caused by the outbreaks of small-pox in the State, and by the prevalence in some places of diphtheria. In one northern township, where the health officer is not a physician, the local officers applied to this office for a physician to be sent to aid in stopping the spread of the disease; and by direction of the Secretary of the State Board, Dr. Hawkhurst, ex-health officer of West Bay City, went there at the expense of the township, and has reported that the outbreak has been stopped.

The usual number of complaints have been received of sick-

ness caused by flooding rivers for the purpose of running logs in the northern part of the State. In answering these the Secretary has used Mr. Parker's paper on the powers and duties of local boards of health.

EXAMINATION IN SANITARY SCIENCE.

The fee for examination in sanitary science was changed from \$10 to \$1, the latter sum being deemed sufficient to cover the actual expense. It was voted that applicants unable to be present at this meeting may be examined at the meeting of the Board, October 11, 1881, application to be made to the Secretary, at Lansing.

GLUCOSE SUGAR AND SYRUP.

Several samples of sugar and syrup, manufactured at the Michigan Grape and Sugar Factory, at Detroit, were presented to the Board and partially examined.

NOTICES OF CONTAGIOUS DISEASES TO SCHOOLS.

The Secretary presented samples of notices of contagious and infectious diseases sent by the health officer of Grand Rapids and Tecumseh to the superintendents of schools in those cities, and suggested that if the officer of each city would send such notices to superintendents, it would be a very important public health measure.

PREVENTION OF BOILER EXPLOSIONS.

An account of an experimental boiler explosion, by Dr. T. Lawson, was presented. His view is, that they can be prevented by such a construction of the boiler as will stop the too rapid increase of steam under suddenly-reduced pressure, as at starting the engine, or by the sudden introduction of cold water. Results thus far seem to demonstrate the correctness of his theory.

The Secretary was directed to present hereafter, at each meeting of the Board, a *résumé* of the action of other State boards of health.

After auditing bills and accounts, etc., the Board adjourned, to meet October 11, 1881.

Reviews and Book Notices.

ARTICLE X.—A PRACTICAL TREATISE ON DISEASES OF THE SKIN. By Louis A. Duhring, M.D., Professor of Diseases of the Skin in the Hospital of the University of Pennsylvania, etc. Second edition, revised and enlarged. 8vo, pp. 644. Philadelphia: J. B. Lippincott & Co., 1881.

The revised and enlarged second edition of this work, whose previous issue greatly commended itself to the profession, appears with emendations and additions which largely enhance its practical value. The too prevalent fashion of preparing second editions of a work for the press, has here evidently found no favor. Those who have had reason to know the conscientious and scrupulous care with which the author has devoted himself to the other labor he has set before him, will be prepared to find in these pages a further proof of the excellent quality of his work.

Respecting the more frequently occurring diseases of the skin, the special excellence of this volume has long been recognized in the accuracy of its definitions, in the simplicity and fidelity of its clinical histories, in the absence of untenable theories and of an intolerably redundant nomenclature, and in the value of the therapeutic measures recommended both with a view to general and topical treatment. Naturally, in none of these particulars does the new edition fall short of the excellence of its predecessor. We look over its leaves and find them laden with the fruit of ripe experience—a wise discrimination between observation and theory, and a comprehensive familiarity with the literature of each subject presented.

As regards the diseases of less frequent occurrence, those which more commonly fall under the observation of the expert, we find

that the chapters on dysidrosis, and pompholix, hæmatidrosis, scleroderma, morphœa, atrophia cutis, hypertrophy of the hair, atrophy of the hair, scrofuloderma, syphiloderma, and carcinoma have been those chiefly enlarged by important additions; and it is worthy of note that on those subjects, the contributions of the author to current literature have been in many cases of the greatest value.

The new articles all embrace subjects of special interest to Americans, as the rare diseases to which the attention of the reader is called are yearly of more frequent observation here, probably not because they are of increasing frequency, but because the labors of American dermatologists have directed such attention to them that they are more frequently recognized. It was said of Thoreau, by Mr. Emerson, that he expected one day to see the *Victoria regia* growing in the vicinity of Concord, and certainly the number of cutaneous diseases recognized abroad and hitherto supposed to be of the greatest infrequency in America, is rapidly diminishing. The new descriptions found here, relate to uridrosis, phosphorescent sweat, urticaria pigmentosa, dermatitis circumscripta herpetiformis, pityriasis maculata et circinata, dermatitis exfoliativa, dermatitis papillaris medicamentosa, dermatitis gangrænosa, dermatitis capillitii, fungoid neoplasmata, tuberculosis cutis, podelcoma, ainhum, perforating ulcer of the foot, and myoma cutis.

Doubtless there are those who will glance through a list of these names and be conscious of a sensation of dismay. The terms are many of them unfamiliar to the fairly-well educated physician. The subjects to which they refer are to him of small practical moment, he may conclude. Possibly he will dismiss the whole with a too common reflection, that dermatology is wedded to its cumbrous nomenclature and mysterious refinements above the level of the average intelligence. It is worth while to consider this reproach for a moment.

For it is a reproach which has not lacked public expression. The annual meetings of the American Dermatological Association have been criticised in our medical journals, in consequence of the time thus spent in discussion of rare forms of cutaneous disease; and it is sufficiently common to hear medical men of

position and education admit their ignorance of the general subject of skin diseases, attributing this to the large number of rare forms of such maladies and the confusion existing with respect to their designation.

Now, without stopping to charge these sins of nomenclature upon all other departments of scientific labor, we cannot fail to remember that, especially of late years, the terms employed in chemistry and perhaps more particularly in connection with new operations upon the female genitals, have been of the most complicated and perplexing character; it is, however, needful to note that the newer terms employed by the dermatologist are those connected with distinct pathological processes or the objective evidences of such processes, while the others to which we have referred are more generally the terms demanded by the refinements of art, or by the requirements of exactness in technical expression. To know the rule in dermatology, one must know the exceptions; to be able to pronounce with precision upon the most common of diseases, one must be positive in the exclusion of every other. The exceptions, though rare, are visible alike to physician and patient, and clamorously assert their identity in their behavior. They betray their differences also in public, and to the eye of the individual who is not consulted as to their nature. Again, it is always necessary to get rid of the doctrines respecting ghosts, before one is in position to discuss a fact of nature. To establish the fact of a new or rare cutaneous disease, one is placed under the necessity of clearing away an incredible quantity of rubbish.

Look for a moment at the title given here "Dermatitis Exfoliativa." The other names mentioned by our author under this head, as employed by writers, are, "General Exfoliative Dermatitis," "Recurring Exfoliative Dermatitis," "Desquamative Scarlatiniform Erythema," "Recurrent Acute Eczema," "Acute General Dermatitis," "Recurrent Exfoliative Erythema," and "Scarlatiniform Exfoliative Dermatitis." Here unquestionably there is need of a single term, by which may be understood the varieties of exanthem described by such writers as Baxter, Féréol, Fagge, Pyc-Smith, and, among the Americans, by such writers as Bulkley and G. H. Fox. Our author might have

added here, the names also of Guibout, of Paris, and Jamieson, of Edinburgh, had the recent papers of these last-named gentlemen appeared before the book went to press. Surely this is a subject requiring attention in a treatise devoted to the consideration of cutaneous disease.

Here, again, we find the evidences of our author's conservatism. He restricts the use of the term we have given, to certain unusual and occasionally grave forms of disease, characterized by an acute erythematous, more rarely vesicular or bullous, inflammation, localized or generalized, accompanied by a more or less marked febrile disturbance, accompanied or followed by varying degrees of desquamation or exfoliation of the epidermis, and marked by a tendency to relapse. The disorder he thus describes is to be distinguished from the recognized varieties of eczema and psoriasis, and from pityriasis rubra and pemphigus foliaceus. But how are these distinctions to be established, especially in face of the fact, that "much diversity of opinion exists as to the true nature of the cases * * * and it is difficult to determine from the reports whether they illustrate the same process or different diseases." Difficult, indeed, it is; one is strongly tempted to disregard alike authorities and minor differences of fact under the circumstances, and to agree with Jamieson that the term "general exfoliative dermatitis" should be made to include the very diseases from which we are here told that the conditions included by that term should be carefully differentiated.

Now it is in just such periods, when our knowledge of the different phenomena displayed under different circumstances by a single pathological condition is widening and deepening, that a wise conservatism is requisite. True, there is a wide distinction between typical cases of pemphigus foliaceus, for example, where the dried contents of a collapsed bleb furnish the material of the papery flakes cast off from the surface, and that disorder characterized merely by "bullous inflammation followed by various degrees of desquamation of the epidermis with a tendency to relapse." Yet the evidences of the existence of intermediate forms between these extremes are not wanting, and Jamieson (*Edinburgh Med. Journ.*, April, 1880, p. 879) cites several cases which it would be exceedingly difficult to assign exclusively

to the one category or the other. One such has also been reported by ourselves (CHICAGO MED. JOURN. AND EXAM., February, 1881), under the title *Pityriasis Rubra*, where there was coexistence of very extensive epidermal desquamation, with febrile accesses of fatal termination, and where the surface exhibited marble-sized projections, which were found, post-mortem, to contain a creamy pus, and which served to give a particular character to the malady. We are not yet ready for a distinct recognition of all these rare and severe cutaneous manifestations as members of a single family, and yet, if the signs do not fail, to that complexion we shall eventually come. Our author has wisely given us a provisional recognition of these typical states, under the title "*Dermatitis Exfoliativa*."

We could in this way go over all the interesting ground traversed here for the first time, by one well qualified to pronounce with decision at every step, but we must be content by reminding those who find it necessary to consult a treatise of this sort, of the importance to themselves of possessing a brief yet precise description of each of the rarer cutaneous diseases, especially when it is requisite to come to a diagnosis by the processes of exclusion. It is for these reasons that we take especial pleasure in commending Dr. Duhring's book to the practitioner as well as to the student of medicine, and it is probably true that the marked success of the first edition was based upon its value in just this particular. Of it can be said now, even more confidently than on its first appearance, that it is the best treatise in the English language on the subject of diseases of the skin. As a text-book, it stands without a rival, and as a monument to the author's painstaking and indefatigable industry, it is one upon which he can well be congratulated. It is the production of books like this, which has brought to the profession in this country a degree of recognition and respect abroad which we could not in any other way have attained, and it is this which has held up to our own countrymen a standard of literary excellence in authorship which has proved of inestimable value.

The new illustrations in the chapter on anatomy of the skin are very creditable drawings by Dr. Van Harlingen, and serve well to exhibit the relations between the protoplasmic bodies in

the mucous layer, formerly described as hanging together by spines or prickles at their points, and clearly shown by Heitzmann to be generally connected by a thread-like network of living matter, traversing the lifeless cement substance between the epithelia.

The typography of the book is beyond criticism, and in other respects the publishers have done their part in its production with a most commendable success.—*American Journal of the Medical Sciences*.

J. N. H.

ARTICLE XI.—LECTURES ON DISEASES OF THE NERVOUS SYSTEM, ESPECIALLY IN WOMEN. By S. Weir Mitchell, M.D., Member of the National Academy of Sciences (and of ten medical societies), etc. With five plates. 12mo, cloth, pp. 238. \$1.75. Philadelphia: Henry C. Lea's Son & Co., 1881. Chicago: Jansen, McClurg & Co.

This is a very ill chosen title for a contribution to the study of some phenomena related to hysteria, chorea, and epilepsy. The five lithographic plates are five tables exhibiting the barometric pressure, atmospheric temperature, and other conditions of the weather in their relation to chorea and infantile palsy. They are of doubtful value. This monograph is pleasantly written, contains not one technical term, treats of some of the most common events of life; all this with little pretension to scientific researches, and will find its sphere of usefulness as a means of advertising among nervous people, at least this idea will suggest itself to many readers.

The author reports some very interesting cases that came to him from Indiana, Kentucky, Ohio, Mississippi, Canada, Maryland, Rhode Island, Massachusetts, New Jersey, Connecticut, etc., and he cured several of them. One must not judge from this however, that this production of a specialist, and "common sense doctor" is devoid of merits. It treats of some subjects never before so keenly investigated as by Dr. Mitchell, whose manner of treatment is highly commendable and too often overlooked by the "busy practitioner." No doubt it will benefit the latter to peruse in an abstract the substance of these lectures,

since he will not have time to read such prolix literature. The author's words are preserved as much as possible, and so also the order of chapters.

PARALYSES OF HYSTERIA.

Causation.—It is most common among the ill-fed, needy, and worried; people that have lost money, and those who lack distinct purposes and aims in life. The palsy may be partly real, partly pure weakness, partly loss of power from want of belief in being able to move. In practice it is usual to find two or three causes acting to assist one another. Four cases of hemiplegia on the left were met to one on the right, and the face was not usually involved. All the senses are liable to be blunted. Profound emotions act as a cause.

Symptoms.—You will see women with a low, whining, bleating voice that is by itself a tell-tale of the kind of will-less ataxia which seems to cripple the mind no less than the body. These are the hard cases to relieve. In fact, even with the best of self help from the patient, the cure of any one of these cases is a long and arduous course of education. All hysteria is not after a time within control of the sick person, and no human sagacity will suffice to enable us to predict results from profound emotions on the patient.

Treatment.—The cures of these cases are to be made by a slow, steady, hopeful training of the will powers through every-day effort, which needs some caution not to err in the way of excess. There is no short cut; no royal road.

HYSTERICAL PARESES.

In some cases of this, nerve conduction is retarded, and gives rise to such incoördinate action as they exhibit. This is made worse by excluding the guiding influence of vision. It is limited for the most part to the more complex movements. This is common in grave hysteria.

Course.—The history of hysteria is sometimes one of years, and in certain cases, either at the outset or after more or less of the strange drama of this disease has been played, the patient falls into a state of inertness of mind and body, which I am forced, for lack of a better name, to call hysteric paresis. This is one of those

disorders which adds many recruits to that large class which some one has called "bed cases," and which are, above all things, distinguished by their desire to remain at rest. They are liable to have relapses.

Treatment.—In grave cases it is best to teach the patient to creep, which is an easy and natural mode of training for the walk. The progress to creeping is easy, then comes the lesson of kneeling and pushing a chair; and last, that of standing in a corner or by a chair. No drug has a direct value. An occasional benefit is derived from induction currents. What you have to do is to rectify with care positive uterine troubles, to treat defects of nutrition, to relieve the anæmia so apt to exist in hysteria, to see that every function is well cared for; and last, not least, to learn what need there is to alter the moral surroundings of your patient, and then with kind and patient care, and an unbinding will, to bring about the changes she may seem to require.

MIMICRY OF DISEASE.

Etiology.—The elements out of which these disorders arise are deeply human, and exist in all of us in varying amount, while many of the determining and conditioning factors come from accidental, or, at least, external agencies. The hysterical state, the nervous temperament, general nervousness, neurasthenia. In the over-sensitive, emotional and timid. It is with some people a morbid birth-gift, with some an inheritance, and in its worst shapes it is made or acquired by misuse of alcohol or tobacco, or tea or coffee. It comes from failing health. Such a state is usually slowly created; but a permanent state of general nervousness may be evolved by the accident of a moment, when precedent conditions favor it. Then every one has some capacity for mentally influencing or disturbing functions of the body which usually are not under the control of volition. In some hysteric or nervous states this power becomes enormously increased. There exists in all of us, feebler in age and more potent in childhood, a tendency to automatic and unconscious imitation which is the parent of a good deal of the mimicry of disease. In this disease, indeed, we find women, and men too,

passing into a mental state in which they are really much like people in dreams. Their power to reason on the phenomena of the senses leaves them, and what they conceive to be the case takes the place of that which is. This is to be met with, to some extent, in all grave hysteric cases. In these cases the emotions are no more under control than in a dream, and the pains are little. A case recovered after a few days, but sometimes the return to health and healthy ideas exacts a long and tiresome trouble. Some children enjoy the important rôle of being ill.

Treatment.—The need for care in discussing symptoms before nervous women or children, the necessity of early apprehension of the true state of things in simulated disease, and the wisdom of acting decisively when once we are sure of our ground are obvious.

Unusual Forms of Spasmodic Affections in Women.—The woman who habitually has hysterical spasms has something wrong with her general health. She is anæmic, or has lost general tone; there is nervous exhaustibility. If we can relieve it we cure the convulsions.

Tremor. Chronic Spasms.—Tremor is an expression of weakness in old age, fatigue, convalescence, organic diseases, or toxic states, transient emotions, fear, excitement, anger, or grief; disappointed love, abuse of alcohol, etc. It is most persistent as a result of nervous wounds. There is no abrupt recovery. The treatment largely consists in experimenting with these cases. Hypodermic injection of atropia locally in nervous spasms is useful. So is massage, extension by splints and apparatuses.

Chorea of Childhood.—Stormy weather, spring time and schools influence the disease, which has a tendency to recur. Puberty is causative also. This is essentially a disease of cities, and is rare in the Negro. Arsenic is the best remedy.

Habit Chorea—also a disease of childhood—consists in rapid and repeated twitching of some muscles; rapid winking, shrugging of the shoulder, etc. Most common in girls from seven to fourteen years of age. There has been some fall from the plane of health. A cause cannot always be detected. The child's restraining power is not well exercised. Good and careful diet,

light gymnastics, no school, gentle aperients, full doses of arsenic, form the best treatment. Hypodermic injections of Fowler's solution without the lavender, thrice a week, in doses rising from two to twelve drops, is the best method of administration.

Disorders of Sleep in Nervous or Hysterical Persons.—This corresponds as much to epilepsy as habit chorea does to chorea. Self control, and some fatigue will relieve it.

Vaso-motor and Respiratory Disorders in the Nervous or Hysterical.—The *pulse* is apt to be as rapid as 90, 100, 130 a minute, and the extremities cold. Flushing with tumultuous heart action, and semi-lateral sweating. The *respiration* has been observed at 10, 15, and even 96 in a minute. Loss of power in the crico-arythenoid muscles is the common type of hysterical aphonia. The patient may be able to sing well, although unable to speak. She may speak aloud during sleep. It is well to recommend them to speak with a full chest. Tonics, rest, and full feeding are indicated. Relapses are common. There is a sense of choking, and difficulty to swallow. This has led some to observe long fasts and not take food nor liquid for as long as twenty-seven days.

Gastro-Intestinal Disorders of Hysteria.—Cases of persistent hysterо-epilepsies, and of severe contractions, are not frequent in this country. The author observed five cases of hysteria lasting from fifteen to twenty-five years, all bed-ridden. Few cases died. Feed the patient, empty the bowels, give exercise, baths, tonics. It is nearly always some trick of the stomach or gastrointestinal tract which sooner or later baffles or perplexes us. Anorexia, regurgitation, spasms on taking food by the mouth or by the rectum. Seclusion, massage, skimmed milk diet, are all useful. Absence of all possible use of body and mind. Iron in large doses, and cod-liver oil, when well borne, are useful in hysteria with anæmia. The hysteria of puberty is the most favorable. But women in good condition, fat and ruddy, with sound organs and good appetites, but ever complaining of pains and aches, and liable to a variety of hysterical phenomena, cannot usually be cured.

H. D. V.

Items.

OFFICIAL LIST OF MEDICAL OFFICERS AND ACTING ASSISTANT SURGEONS of the United States Marine Hospital Service, with their Stations, July 1, 1881:

Supervising Surgeon-General.—John B. Hamilton, Washington, D. C.

Surgeons.—Ernest Hebersmith, San Francisco, Cal.; P. H. Bailhache, National Board of Health; John Vansant, Boston, Mass.; W. H. H. Hutton, Detroit, Mich.; T. W. Miller, Chicago, Ill.; Walter Wyman,* Cincinnati, Ohio; W. H. Long, Louisville, Ky.; R. D. Murray, Memphis, Tenn.; C. S. D. Fessenden, New York, N. Y.; George Purviance, Pittsburgh, Pa.; H. W. Sawtelle St. Louis, Mo.; H. W. Austin, New Orleans, La.; J. M. Gassaway, Philadelphia, Pa.

Passed Assistant Surgeons.—Henry Smith, Norfolk, Va.; G. W. Stoner, Portland, Me.; J. C. Fisher, Washington, D. C.; John Godfrey, Mobile, Ala.; C. B. Goldsborough, Baltimore, Md.

Assistant Surgeons.—Fairfax Irwin, Wilmington, N. C.; F. W. Mead, Port Townsend, W. T.; H. P. Cooke, Galveston, Texas; H. R. Carter, Cairo, Ill.; W. H. Heath, Buffalo, N. Y.; F. J. O'Connor, Evansville, Ind.; F. D. Porter, Chicago, Ill.; John Guiteras, Key West, Fla.; W. A. Wheeler, Charleston, S. C.; J. A. Benson, St. Louis, Mo.; C. E. Banks, San Francisco, Cal.; D. A. Carmichael, New York, N. Y.; S. T. Armstrong, New Orleans, La.

Acting Assistant Surgeons.—J. M. Allen, Milwaukee, Wis.; W. B. Banks, Rockland, Maine; H. G. Bates, New Berne, N. C.; C. C. Berry, Portland, Maine; R. D. Bibber, Bath, Maine; F. N. Blount, Pensacola, Fla.; J. E. Bready, Dubuque, Iowa; G. B. Case, Cleveland, Ohio; Byron DeWitt, Oswego, N. Y.; L. B. Edwards, Richmond, Va.; A. W. Fisher, Toledo, Ohio; J. P. C. Foster, New Haven, Conn.; W. M. Griffiths,* Louisville, Ky.; A. C. Hamlin, Bangor, Maine; L. W. Hodgkins, Ellsworth, Maine; S. B. Hunter, Machias, Maine; J. M. Kercheval, Nashville, Tenn.; H. E. Mereness, Albany, N. Y.; J. D. Mitchell, Jacksonville, Fla.; Charles Otilie, La Crosse, Wis.; R. H. Pease, Chicago, Ill.; C. T. Peckham,† Boston, Mass.; W. H. Pope, Fernandina, Fla.; T. T. Price, Tuckerton, N. J.; S. D. Robbins, Vicksburg, Miss.; Elmer Small, Belfast, Maine; J. G. Stanton, New London, Conn.; W. D. Stewart, Vineyard Haven, Mass.; G. H. Stone, Savannah, Ga.; D. H. Strickland, Erie, Pa.; H. S. Taft, Marquette, Mich.; W. H. Taylor, New Bedford Mass.; J. H. Vandeman, Chattanooga, Tenn.; M. F. Wentworth, Portsmouth, N. H.; C. A. Wheaton, Jr., St. Paul, Minn.; J. E. Wood, Elizabeth City, N. C.

* Temporary duty as medical officer revenue-bark "Chase."

† Temporary duty at Cincinnati during the absence of Surgeon Wyman.

‡ Temporary.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF MEDICAL OFFICERS of the U. S. Marine Hospital Service, April 1, 1881, to June 30, 1881:

Bailhache, P. H., Surgeon. Detailed as Chairman Board of Examiners. April 5, 1881.

Wyman, Walter, Surgeon. Detailed for temporary duty as medical officer revenue-bark "Chase." May 31, 1881.

Long, W. H., Surgeon. Detailed as member Board of Examiners. April 5, 1881.

Murray, R. D., Surgeon. To proceed to Memphis, Tenn., assume charge of the service at that port, and inspect the service at Vicksburg, Miss. April 8, 1881.

Purviance, George, Surgeon. Detailed as Recorder Board of Examiners. April 5, 1881. To proceed to Richmond, Va., as inspector. April 30, 1881. Detailed as Chairman Board of Survey, to examine applicants for admission to the Revenue Marine Service. May 10, 1881.

Doernig, E. J., Surgeon. Granted leave of absence for thirty days from April 21, 1881. April 2, 1881.

Austin, H. W., Surgeon. Granted leave of absence for thirty days from May 5, 1881. April 2, 1881.

Gassaway, J. M., Passed Assistant Surgeon. To proceed to Philadelphia, Pa., and assume charge of the service, relieving Passed Assistant Surgeon Stoner. April 7, 1881.

Smith, Henry, Passed Assistant Surgeon. To report to Chairman Board of Examiners. April 25, 1881.

Stoner, G. W., Passed Assistant Surgeon. To proceed to Portland, Maine, and assume charge of the service, relieving Surgeon Doering. April 7, 1881.

Fisher, J. C., Passed Assistant Surgeon. Detailed as Recorder Board of Survey, to examine applicants for admission to the Revenue Marine Service. May 10, 1881.

Wheeler, W. A., Assistant Surgeon. Granted leave of absence for thirty days (he providing a substitute,) from July 7, 1881. June 23, 1881.

Carmichael, D. A., Assistant Surgeon. To proceed to New York, N. Y., and report to Surgeon Fessenden for duty. April 7, 1881.

RESIGNATION.

Doering, E. J., Surgeon. Resignation accepted by the Secretary, to take effect May 20, 1881. April 2, 1881.

PROMOTION.

Gassaway, J. M., Surgeon. Promoted to be Surgeon from May 21, 1881. May 16, 1881.

EXAMINATION PAPERS OF THE ILLINOIS STATE BOARD OF HEALTH.

CHICAGO, JUNE 29, 1881.

Obstetrics—By A. L. Clark, M.D.

1. Give the mechanism of the Occipito-Sacral position.
2. Define placenta previa, and give—*a.* Its diagnosis. *b.* Prognosis to mother and child. *c.* Treatment.

3. How will you Diagnose Pregnancy from Ovarian Tumor?
4. Give the contra-indications for the use of Ergot.
5. How can you diagnose Fibroid Uterine Tumor from Flexion of the Uterus?
6. What blood-vessels are contained in the Umbilical Cord?
7. What are the sympathetic signs of Pregnancy?
8. Give Pathology, Diagnosis, Prognosis and Treatment for "hour glass contractions" of the Uterus.
9. What stage of labor is attended with the greatest danger, and what is the danger?
10. Give the dangers in—*a.* The first stage of labor. *b.* The second stage of labor. *c.* The third stage of labor.

Chemistry—By A. L. Clark, M.D.

1. What is the number of Simple Chemical Elements?
2. What is Equivalent Weight?
3. Give the Chemical Symbol for Antimony, and give an Antimonial Preparation sometimes used in medicine.
4. Explain the combustion and flame of a candle.
5. Under what circumstances will a galvanic current be formed?
6. What is the specific heat of any body?
7. What takes place when any soluble Salt of Iron is prescribed or mixed with most vegetable Infusions or Tinctures?
8. Give the Chemical Symbol for the Monoxide of Nitrogen, and suggest the uses of the compound.
9. What is Specific Gravity?
10. Name ten Elementary Substances.

Gynecology—By R. Ludlam, M.D.

1. What is the most frequent of all the Uterine displacements?
2. Give the diagnosis of Hysteria from Epilepsy.
3. How would you know a case of Ulceration of the Uterine Cervix from one of Cervical Endometritis?
4. What are the symptoms of Laceration of the Cervix?
5. What is the prognosis in Pelvic Cellulitis?
6. When, if ever, do Fibroid Tumors become malignant?
7. Is Leucorrhœa a symptom or a disease?
8. Give the symptoms and treatment of Prolapse of the Ovary.
9. What are the indications for the use of the Sponge Tent?
10. Name the diseases in which Menorrhagia is an almost invariable symptom.

General Pathology—By R. Ludlam, M.D.

1. What is Anæmia?
2. Give the general pathology of Fever.
3. What is the difference between Hypertrophy and Atrophy?
4. What do physicians understand by compensating Hypertrophy? Give an illustration.
5. Define the word diathesis, and name the most important Diatheses.
6. Describe the different stages of Cerebral Apoplexy.

7. Is Dropsy a symptom or disease, and why?
8. What is the pathology of Hemi-anæsthesia?
9. In the hepatization of Pneumonia, if chloride of sodium is deficient in the urine, where do we find it?
10. In intermittent Fever when would you apply the thermometer in order to get the highest range of temperature?

Hygiene—By J. H. Rauch, M.D.

1. What kinds and what amount of food should be furnished those sick with acute diseases? When and how should it be given?
2. What are disinfectants? Name the best. How do they act as such? How and when should they be used?
3. How would you prevent or limit the spread of Typhoid Fever, Diphtheria, Scarlatina?
4. In a case of Typhoid Fever, where would you look for its probable source or origin?
5. What effect would the drainage of the level prairies of the State have upon health?
6. How would you ventilate the school room? How much air space should each pupil have?
7. How would you resuscitate from the asphyxia of Drowning, Coal Gas, Chloroform or Ether?
8. What is Trichinosis? How can it be prevented?
9. Which do you prefer, humanized or non-humanized vaccine virus, and why?
10. How would you diagnose Small Pox from Measles, Scarlet Fever, Chicken Pox?
11. How would you prevent the spread of Small Pox?
12. How would you render privies and water closets innocuous?
13. How do you account for the increased mortality of the past six months?
14. What effect, upon the public health, will result from the recent overflows in this State?
15. Why should the registration of Births, Deaths and Marriages be enforced?

Medical Jurisprudence—By J. H. Rauch, M.D.

1. In a case involving legal questions, how would you conduct a post-mortem examination?
2. How would you conduct the examination of an insane person for commitment to an insane asylum?
3. State under what circumstances you would deem it necessary to hold a coroner's inquest.
4. How would you differentiate between Drunkenness and Apoplexy?
5. To what extent are physicians liable for the bad results of treatment of a fracture?

Practice of Medicine—By John McLean, M.D.

1. Is Pneumonia a specific fever, or a local inflammation with fever as a result? Give reasons why.

2. What are the varieties of Pneumonia?
3. How would you treat a typical case of Catarrhal Pneumonia?
4. How would you diagnose Bronchitis from Pneumonia?
5. How would you treat Bronchitis?
6. How would you diagnose Pleuritis from Bronchitis? From Pneumonia?
7. How would you treat Acute Pleuritis?
8. What would be your treatment if effusion followed Pleuritis?
9. What is Diphtheria? How would you treat a case?
10. How would you diagnose Measles from Scarlet Fever?
11. How would you treat Measles?
12. How would you treat Scarlet Fever?
13. What are the symptoms of Acute Gastritis?
14. How would you treat Dysentery?
15. How would you diagnose a case of Morbus Brightii?
17. What pathological condition is found in Cerebro Spinal Meningitis?
18. How would you treat a case?
19. What is the supposed cause of Malarial Fever?
20. How would you treat a Remittent Fever?

Materia Medica and Therapeutics—By J. H. Rauch, M.D.

1. What are—[1] Infusions, [2] Tinctures, [3] Fluid Extracts, [4] Waters, [5] Solutions?
 2. What are—[1] Ointments, [2] Cerates, [3] Suppositories, [4] Spirits, [5] Oleo-Resins?
 3. Give full Latin officinal name, opium strength, and doses of—[1] Black Drop, [2] Dover's Powder, [3] Laudanum, [4] Extract of Opium. [5] How does the Deodorized Tincture of Opium differ from Laudanum?
 4. [1] Name and give the origin of the three officinal kinds of Aloes. [2] Which sort is best for medical use, and why? Name and give doses of three Aloëtic preparations. [4] Give the composition of Compound Cathartic Pills.
- Sources, active principles, three preparations, and doses of each of the following drugs:
5. Belladonna
 6. Capsicum.
 7. Cinchona.
 8. Senna.
 9. Cinnamon.
 10. Dose of—[1] Acid Hydrocyanicum Dilutum, [2] Camphora, [3] Chloral, [4] Elaterium, [5] Ext. Ergotæ Fluid, [6] Ext. Hyoscyami, [7] Iodoformum, [8] Oleum Ricini, [9] Tinctura Aconiti Radicis, [10] Tinct. Veratri Viridis.
 11. Various methods of treating acute rheumatism. Expectant, Alkaline, Salicylic, Theories and result.
 12. Giving details of carrying out the above.
 13. Hypnostics—Opium, Chloral, Bromides, Lactucarium—Peculiarities of each.

14. Cathartics—Mention five different kinds. State peculiarities of action, and occasions on which you would wish to use each.

15. Name the Cinchona Alkaloids. What are their comparative values?

Physiology.

1. What is the specific gravity of normal urine?
2. How is gastric juice produced?
3. How is the peristaltic motion of the bowels produced?
4. How many sounds has the heart? And how do you distinguish them?
5. Is bile a recrementitious or excrementitious product, or both?
6. What are the functions of the pneumogastric nerve?
7. Describe the difference between a serous and mucous membrane.
8. What organ of the body commences growing about the 45th year?
9. What function does the pancreatic juice perform?
10. Why is venous blood darker than arterial blood?

Anatomy.

1. Name the principal fluids of the body.
2. Describe the circulation of the blood in the adult.
3. State the number of bones composing the human skeleton, and name those of the cranium, face and vertebral column.
4. State number and names of the several muscles of the eye?
5. What are the principal divisions of the human brain?
6. Name and describe the membranes of the brain and spinal cord.
7. State number and names of the cranial nerves.
8. Describe the sympathetic nerve.
9. What structures are cut and what artery is endangered in the operation for strangulated hernia?
10. Describe the regions of the abdomen, and state their contents.

Surgery.

1. Define malignant and benign tumors, and give examples of each.
2. How diagnose dislocation of humerus from fracture?
3. How reduce subglenoid dislocation of humerus?
4. State how you would diagnose, dress and treat a simple fracture of the leg.
5. When is a wound said to heal by first intention, and what conditions are necessary to such a result?
6. What are the indications for tracheotomy, and the dangers attending the operation?
7. What would you do in fracture of the skull without signs of depression?
8. What conditions and indications would demand the amputation of an injured limb?
9. Give general directions for local and constitutional treatment after amputations?
10. How would you administer the anæsthetics, ether and chloroform, and what do in case of suspended respiration during their administration?

SOCIETY MEETINGS.

Chicago Medical Society—Mondays, Sept. 5-19.

West Chicago Medical Society—Mondays, Sept. 12-26.

Biological Society—Wednesday, Sept. 7.

CLINICS.

MONDAY.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Prof. Holmes; 3 p. m., Otological, by Prof. Jones.

Mercy Hospital—2 p. m., Surgical, by Prof. Andrews.

Woman's Medical College—2 p. m., Dermatological and Venereal, by Prof. Maynard; 3 p. m., Diseases of the Chest, Prof. Ingals.

TUESDAY.

Rush Medical College—3 p. m., Dermatological and Venereal, by Prof. Hyde.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Quine.

WEDNESDAY.

Chicago Medical College—2 p. m., Eye and Ear, by Prof. Jones.

Rush Medical College—2 p. m., Medical, by Dr. Bridge; 3 p. m., Ophthalmological and Otological, by Prof. Holmes; 3:30 to 4:30 p. m., Diseases of the Chest, by Dr. E. Fletcher Ingals.

THURSDAY.

Chicago Medical College—2 p. m., Gynæcological, by Prof. Jenks.

Rush Medical College—2 p. m., Diseases of Children, by Dr. Knox; 3 p. m., Diseases of the Nervous System, by Prof. Lyman.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Dr. Hotz.

Woman's Medical College—3 p. m., Surgical, by Prof. Owens.

FRIDAY.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Davis.

SATURDAY.

Rush Medical College—2 p. m., Surgical, by Prof. Gunn; 3 p. m., Orthopædic, by Prof. Owens.

Chicago Medical College—2 p. m., Surgical, by Prof. Isham; 3 p. m., Neurological, by Prof. Jewell.

Woman's Medical College—11 a. m., Ophthalmological, by Prof. Montgomery; 2 p. m., Gynæcological, by Prof. Fitch.

Daily Clinics, from 2 to 4 p. m., at the Central Free Dispensary, and at the South Side Dispensary.



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Original Communications.

ARTICLE I.

IS CRANIOTOMY EVER JUSTIFIABLE? By A. REEVES JACKSON, A.M., M.D., etc. Read before the Chicago Gynæcological Society, December 17, 1880.

Mr. President and Gentlemen:—I regret that I am obliged to commence with an apology. I have to present you with a very brief and incomplete paper,—its brevity being, however, its greatest merit. But, thinking it better to do my duty partially than not do it at all, I have brought the paper before you “scarce half made up.”

I ask your attention to the consideration of the question:

IS CRANIOTOMY EVER JUSTIFIABLE?

The act of parturition in the human female must always have been one of the most interesting character; and, although we have no facts upon which to base the opinion that the process was other than physiological and safe in the very early history of our race, it seems probable that many causes must have soon come into action which were certain to occasion anxiety and danger.

Out of the perils and difficulties which came to attend parturition arose the art of midwifery. And, as we might expect, this concerned itself chiefly with the cases in which delivery could not be completed without artificial aid. When the passage of the child became obstructed nothing could be more natural than to lessen its size, and extract it by means of the hand, hooks and pincers. Accordingly, we find that the earliest obstetrical operation of which we have any mention is that of craniotomy. This operation is spoken of very fully by Hippocrates and other early writers; and, inasmuch as both turning and the use of the forceps were unknown to them, craniotomy was probably practiced in many cases where these alternatives would have been employed at a later day.

Embryotomy holds the peculiar relation to surgery that it is the only operation which is undertaken and sanctioned by the medical profession with the avowed intention of destroying a human life. All other obstetrical operations—version, forceps, Cæsarean section and other forms of laparotomy—afford a chance of safety to both mother and child. Nevertheless, this apparently cruel practice has been justified upon what seem strong and proper grounds. Even now, however, there are some conscientious practitioners who hold that the mutilation of the child is only justifiable after its death has been positively ascertained.

The common sentiment upon this point is, that when the child, living or dead, cannot be removed from the mother by the more ordinary methods,—forceps or turning,—and the dimensions of the natural pelvis are such as to permit its delivery by lessening the size of the foetal head, such mutilation is proper and commendable; and that the life of the child, under such circumstances, should be subservient to that of the mother. This doctrine is held and promulgated, with perhaps a very few exceptions, by the leading obstetricians throughout the world.

A few months ago a somewhat remarkable paper was read before the Obstetrical Society of Dublin, by Dr. R. J. Kinkead,* in which the author takes very decided ground in opposition to these views, and maintains that, if craniotomy be ever justifiable its proper sphere is extremely limited.

* Craniotomy and its Alternatives—Cæsarean Section, Laparo-Elytrotomy, and Porro's Operation. R. J. Kinkead, A.B., M.D.—*Obs. Jl. Gr. Br. and Ireland*, March, 1880.

It is my purpose, in this paper, to notice and briefly comment upon the arguments which have been advanced by the writer referred to in support of his position. In order to do this more conveniently, I have endeavored to embody his views in a series of propositions.

Proposition 1. *The child, whilst in its mother's womb, has a separate, individual existence ; has as perfect a right to its life as its mother has to hers ; that our duty binds us to put its life into the scale against that of a being like ourselves, accountable to the Almighty ; that, in short, we have no right to entertain the question of the relative value of one human life as compared with another.*

A fallacy underlies this argument ; for, while it is true, in the abstract, that one human life is as good as another, yet, when two lives are in imminent peril and one may be saved by the sacrifice of the other,—and in no other way,—it seems highly proper for us to choose which shall be rescued. And this principle of action has always been recognized in the case in question ; scientists and philanthropists have agreed,—generally, at least,—that the life of the mother should be held superior to that of her unborn child, and that if one must perish, it should be the latter. It is conceded that we should preferably save the strong rather than the weak ; the mother, who is bound by many ties, social and moral, to her family and the world, rather than the infant without hopes, fears, affections or responsibilities. So that, while we may admit that human life should be held at the disposal of God alone, we need not forget or ignore the fact that our reason has been given us for the purpose of affording us proper guidance in cases of doubt and difficulty. And when our reason and the experience of centuries teach us that the lives of two of His creatures are mortally endangered, and that we may save one only by prompt and efficient action, we feel justified in saving that which seems to our finite judgment the most valuable.

Proposition 2. *In consequence of the teaching that the safety of the mother is always to be preferred to that of the child, resort to embryotomy has been too frequent ; and attempts have been made to perforate and deliver in pelves so extremely narrow that extraction was impossible, leaving no alternative but to either*

operate subsequently on the mother or let her die undelivered. Hence, such teaching is improper and dangerous.

The truth of this proposition may be fully admitted—and I think it ought to be—yet it is not so much an argument against the performance of craniotomy as against its improper use. Those who have had the best teaching are not necessarily the best taught. Any surgical operation or other remedial measure may be subject to abuse, without the fact militating against the proper and judicious use of it. It is quite conceivable—quite probable, indeed—that the attempts to deliver, which are characterized by Mr. Kinkead as unwarrantable, have not been the result of carelessness but the consequence of inherent difficulties in the case. For example, a pelvis may be sufficiently large to permit the extraction of a child of usual size after mutilation; and the attempt to deliver by craniotomy would, under the circumstances, be quite justifiable, even though it might have to be abandoned subsequently owing to the foetus being larger than common.

Proposition 3. *In the case of a woman in labor, with such a degree of pelvic obstruction as to make it evident that unless speedy aid be given, both mother and child must die, it becomes our plain duty to save one of the two so endangered if we cannot save both; hence, craniotomy being necessarily fatal should not be performed unless we are assured that, with reasonable skill in its performance, the mother's life will be spared.*

This brings us to the consideration of the question, What are the smallest pelvic diameters which will permit the extraction of a dismembered foetus with safety to the mother? Upon this point authorities differ; for, while some state that it cannot be accomplished unless there is a clear space of three and a half by two inches, the majority fix the limit at three by two, and three by two and a half. Barnes, indeed, states that under some circumstances three by one inch may be sufficient.

It is clear that the limit should be such as would be reasonably certain to secure the safety of the mother; for, as one author urges, if craniotomy be attempted in any case in which a secondary resort to another method must be had, the danger to the mother is enormously increased; and two lives may possibly be sacrificed in the attempt to save one. But, to make choice of

operation in many of these cases must be very embarrassing. It is very difficult, perhaps impossible, to ascertain within a fraction of an inch, the diameters of the pelvis, especially during labor, the time when they usually first come under notice. Then there are the considerable differences which may exist in the size of the foetal head,—quite as important an element in the calculation, and one which we can only estimate. According to statistics collected by the late Dr. Parry, of Philadelphia, out of seventy cases of craniotomy where the smallest diameter was two and a half inches or under, the maternal mortality was about one in two and a half, nearly equal to the usual mortality from Cæsarean section.

In comparing the relative mortality of craniotomy and Cæsarean section,—the first being usually placed at one in five, and the latter at one in two and a half,—Mr. Kinkead lays great stress upon the fact that where craniotomy has been performed after long duration of labor—say forty-eight hours—the maternal mortality becomes greatly increased; and that where vain attempts to deliver have been made with forceps, it reaches a mortality of one in two and a half, thus becoming as fatal as Cæsarean section. He proceeds, further, to show that while the tables relating to Cæsarean section show a mortality of about 57.6 per cent. they include cases in which the patients had been long in labor, some actually dying; others suffering from dangerous and deadly diseases; some in whom craniotomy had been tried and failed; and that, hence, a fair opinion as to the rate of mortality which would obtain under the best and most favorable circumstances was not at present attainable. He states, however, that in the few cases in which timely operations were performed the maternal mortality was reduced to 25 per cent. But he does not seem to consider that the tables upon which the mortality of craniotomy cases is based also include unfavorable cases,—cases in which labor has been of long duration, where the patient has been suffering from previous disease, and where unavailing attempts have been made to deliver by forceps, etc., and that if these were all included the operation would make relatively as favorably a showing compared with Cæsarean section as it does now.

Proposition 4. *In carcinoma of the uterus or vagina, a disease which must in a short time put an end to the existence of the mother, we are justified in saving the child, even though it be at the expense of some additional risk to the mother.*

Here the author makes a really strong point, and I coincide with him. The same principle which impels us to save the mother rather than the child under ordinary circumstances, should here urge us to reverse our action. We should save the best. Here, the child, if saved, would probably become a useful member of society, while the days of the mother are numbered. Embryotomy certainly destroys the child, and in this case does not save the mother; at best it could only for a brief time prolong a painful existence; and it has been shown by statistics that where craniotomy has been performed under these circumstances, it has been almost as dangerous to the mother as Cæsarean section. In reply to the argument, that because the mother's chance for living is small we have no right to still further lessen it, Mr. Kinkead aptly replies that this would be true if the child had no right to live.

Proposition 5. *When labor is complicated by the presence of uterine, ovarian or other pelvic tumors of considerable size, which are incapable of being removed or emptied of their contents, such dangerous pressure must be made upon them, even after the fœtal head has been reduced in size, that craniotomy should not be resorted to in preference to some of its alternatives—Cæsarean section, gastro-elytrotomy or Porro's operation.*

Here, too, I agree with Mr. Kinkead. Usually, in these cases, unlike those in which the deformity is bony, all the pelvic diameters are affected, and there is very great difficulty in extracting even the dismembered child. According to Stadfelt, the maternal mortality in such cases reaches 60 per cent., and when we consider that all the children are killed, the position taken by the author seems impregnable.

DR. JACKSON: In closing the discussion, I have but little to add to what has already been said. I want, however, to sincerely thank the members of the society for the very kind manner in which they have received and commented upon the paper in its present crude and unfinished form. I also thank them for

the courteous permission they have given me to revise and complete it at a subsequent time.

It seems well, in these days of changing views and practice, to occasionally come back to the consideration of old landmarks. In doing so we shall find that every change proposed and practiced is not necessarily an improvement. It was because I regarded the views of Dr. Kinkead, upon the subject of craniotomy, as revolutionary and unsound, that I thought it expedient to bring them before this society for the consideration of the latter; and I am pleased to find such a unanimous expression of opinion in favor of the old views and old and tried methods. To what extent the heterodox views of Dr. Kinkead have found sympathy and support in his own country I cannot say; but, I feel like congratulating this society on the fact that they have found no encouragement here.

ARTICLE II.

THE ACTUAL CAUTERY. By T. F. FRANK, M.D., Pittsburgh, Pa.

Within the meaning of the term actual cautery are embraced all methods by which animal tissues are destroyed, by contact with heated instruments, whether the heat is produced by fire directly, or by the aid of the galvanic battery; and the importance of the application consists in the quality of temperature, but not the mode by which it is produced. By the selection of galvanism for the heating of cautery instruments, however, we are better able to secure to them a more uniform temperature, essential for a successful issue of those operations, than can be done by any other process, and so far the galvanic battery has advantages. Its electric influence is no other than that obtained from metal heated by fire.

With all the advantages the electro cautery can be claimed to possess, it is highly overestimated, and not worth the money for an outfit.

During the past twelve years, I have had ample opportunities

to test all the so-called modern improvements in galvanic cautery apparatus in operations on cancers, polypi, hæmorrhoids, and in uterine surgery, but I must confess that many times, when my expensive fancy instruments failed me, I had to fall back upon a clumsy carbon-zinc cell made for me years ago, by an ordinary mechanic, at a moderate price.

Two years ago Mr. Isaac G. H., of Bradford, McKean Co., Pa., presented himself at my house for an operation on piles by galvano-cautery. The tumors were unusually large, and I feared that the Dawson battery I owned would fail me, as it had done in minor cases; and being unwilling to transport the large cell before mentioned to the hospital where the patient had taken a room, I obtained, through the kindness of Dr. D. N. Rankin, of Allegheny City, a Byrne battery, to help me out should the Dawson fail. Both the instruments were in good condition, and produced all the heat any manipulator could obtain from them; but had it not been for the most economical expenditure of the battery force, I could not have completed the operation. Much of the successful issue of this case depended upon the service I obtained through the assistance of Drs. Rankin and R. S. Sutton, which enabled me to give attention to the action of the batteries, which otherwise I would have been obliged to trust to more inexperienced hands.

Some time in July, 1880, I was requested by Dr. Willard, of Allegheny City, to remove for a patient of his, from Toledo, Ohio, the greater part of the neck of the uterus, on account of carcinoma involving the os. The doctor had just purchased a Piffard cautery, and wishing to test its capacity, requested me to use it in this case. I fortunately made some experiments with this battery, after having prepared it according to directions accompanying the instrument, or the operation would have been a failure. The knife to be employed became hardly warm, while I used the manufacturer's formula for acid, but subsequently I obtained better results, and had the patient prepared for the operation. With a far stronger solution than directed by the maker of the battery, I could heat the platinum knives very well, but was unable to maintain a good temperature during the operation, and there was considerable hæmorrhage to contend with.

I consider it bad practice to rely upon the actual cautery as an instrument for the removal of any growth to which access can be had by the knife, the scissors, or any other rapid method of extirpation. The surgeon does not contemplate dealing with the parts to be removed, but to bring the remaining tissues in the most favorable condition for recovery, and for that purpose the actual cautery becomes a most valuable remedy.

There are no tumors to which the actual cautery is applicable but what any ordinary surgeon may remove with the knife first, and control the bleeding temporarily, till the cautery can be applied. In cases of piles, cauterization is most eminently useful, and in all those cases the bleeding may be prevented by clamping the tumor before cutting it off, and before the cautery can be brought to action.

Now comes the consideration, whether for the cauterization of an open, accessible surface, we need the complicated and expensive paraphernalia connected with our electro-cautery instruments, or whether that operation cannot be more simplified? Every surgeon, no matter how obscure his practice, will find occasions to use the actual cautery, but few are able to purchase a battery, at an expense of \$60—or \$85 with the necessary cautery instruments—and at that obtain a very unreliable apparatus, probably never to be used after the first attempt.

Acting upon this suggestion, I have constructed a more simple cautery, which is in every particular reliable, and can be furnished for one fifth the cost, including the burner, for what any electro-cautery battery is sold without the handles, domes and knives.

For the benefit of those who wish to interest themselves in that particular practice, I give the following description of my new cautery, which I have named the Hydrogen Cautery: It consists of a simple closed copper and zinc element, so arranged internally that the hydrogen escaping from the negative element is retained inside the cell, and is rapidly acquiring density, till a pressure of three ounces to the square inch is obtained. By that time the zinc is free from the contact with the acid, and no more gas can be generated till a portion is allowed to escape for the purpose of cauterization.

The instrument has the capacity of one quart, and after being charged with eight ounces of water acidulated with four ounces

of common sulphuric acid, it will perform steady cautery work upon the most bloody tissues during one hour. The *modus operandi* of this cell, after the acid has been introduced, is perfectly automatic.

For the cautery I have constructed a small Bunson burner out of pipe clay, which with the full pressure of the gas will produce an almost invisible flame the size of a pepper-corn. The caliber of it can be reduced to that of a pin's head. The latter I adopt in most all of my operations.

Mr. William S. S., of Duke Center, Pa., was sent to me for an operation on piles, by Dr. Thos. Stuart, of Erie, Pa. He came at the time when I was making comparative experiments in the presence of several medical gentlemen with the Piffard and Dawson batteries and my hydrogen cautery upon raw beef. The experiment conclusively demonstrated that with one charge of my cell, I could cauterize more surface than the other two did unitedly. The two batteries had to be shaken and pumped constantly, and the acid liquid became boiling hot, while I had my cell in the side pocket of my coat, perfectly cool, and performing its work admirably. Mr. S. was operated upon the next day. He had five very large piles. After constricting the tumors by a snare clamp I lately constructed for those operations, I cut them down with a bistoury, and then simply brushed the little flame, held at a proper distance, over it, till the stumps were as dry and of the appearance of sponge. I never obtained as good results from a cautery operation before this. The patient recovered without requiring any attention after the operation, and left the city on a visit to his relatives in Michigan the sixth day.

For the more delicate operations on the cavities of the mouth and nose, where it becomes of great importance to confine the heat to a particular spot during a definite period, to prevent organs like the Eustachian tube and the middle ear from suffering from the heat radiated by the cautery, I have arranged an electric device, by which I can instantly send a small jet of heat, after the burner has been applied cold to the diseased part, under cover of a shell. This maneuver can be accomplished by the motion of one finger; and the heat stopped instantly in a like manner.

I give this paper with a desire to disincumber the actual cautery, and popularize one of the most promising surgical appliances.

ARTICLE III.

VACCINATION: A CONSIDERATION OF SOME POINTS AS TO THE IDENTITY OF VARIOLA AND VACCINIA. BY THOMAS F. WOOD.

The physician of the present day is no longer an anxious inquirer about the minute details of the theory and practice of vaccination. He regards all the difficulties as having been overcome, and he is willing to rest upon the glories which Jenner achieved. Notwithstanding the apparently settled state in which we find vaccination to-day, there are few more intricate studies underlying any subject than the present practice of vaccination.

The original works of Jenner, Woodville, Pearson, Bryce, Willan, Steinbrenner, Gregory, Bonsquet, Waterhouse, Redmond Coxe, and the hundred other carefully prepared volumes which from 1798 to 1843 were read and studied with so much anxiety, are fast disappearing from private libraries, and they are only known to the reader of to-day by the meagre extracts made from them, which are found in the current standard text-books. The literature of vaccination embraced within the dates mentioned above, marked the most brilliant era in medicine, and no richer field of study can be found to-day, than the treatises on vaccination prepared by so many careful writers. He who would be master of the vast questions which go to make up our knowledge of vaccination, cannot do so and neglect the original works of Jenner and his contemporaries. Many of the principles there wrought out stand fast to-day. Many of the questions then anxiously propounded remain undetermined to-day. Many of the fast rules laid down by Jenner, and neglected by a few generations, we gladly return to eighty years after they are announced, because by disregarding them we have been led into error.

We wish we could feel as confident as Professor Lyman that recent studies of the nature of vaccine (CHICAGO MEDICAL JOURNAL AND EXAMINER, July, 1881, p. 1) have given any material weight to the doctrine of identity. He thinks "we are now in

a position to form a clear idea regarding a subject in connection with which there is no longer any place for mystery."

He furthermore says, in the same article :

"The opinion was long entertained vaguely by many physicians that small-pox and vaccinia were entirely distinct, though similar, diseases. This belief was based upon the fact that Jenner did not himself fully comprehend the manner in which these two forms are related. He believed that they were identical in their origin, but he did not demonstrate that identity."

The opinion is still held by eminent vaccinologists of the non-identity of variola and vaccinia. Recent investigations upon the subject have tended to confirm the opinion that the diseases are structurally and clinically different. Surely, in the estimation of most excellent students, the beautiful experiments of the late lamented Ceely, as to the production of vaccinia by the inoculation of cows with variolous matter, are seriously doubted. These experiments by Ceely, of all the series now so famous in the history of vaccination, were the most reliable and convincing, although we think we can show that good reasons exist why we should regard them with distrust, if not reject them entirely. We contend that small-pox and cow-pox are not identical :

1. Because cow-pox has never been converted by any means into small-pox.

2. Because, in the light of the attempts made by many careful and truthful experimenters, to produce small-pox by the inoculation of the cow with small-pox virus, the number of failures on the part of such a very large majority of them, together with the numerous positive denials made of the possibility of success, it may be concluded that small-pox virus is never converted into vaccine by transmission through the cow. The results of Gassner, of Ceely, and Thielé, and Badcock are not left out of consideration in making this statement.

3. Cow-pox does not become epidemic,* nor does it spread by effluvia from the human subject. It is always confined to the

* An Inquiry into the Causes and Effects of Variolæ Vaccinæ. Edward Jenner, M. D., F. R. S., Springfield Edition, 1802, pp. 54 and 55. "It (cow-pox) has been conceived to be contagious among cows without contact; but this idea cannot be well founded, because the cattle in one meadow do not infect those in another, unless they be handled or milked by those who bring the infectious matter with them."

point of insertion in the human subject, never producing a general eruption of vaccine vesicles.

4. Structurally considered, the vesicles of vaccination and the pustules of inoculation are very dissimilar. Clinically considered, the course and duration of the two diseases are not unlike, but certainly not identical, having enough points of difference to enable the skilled diagnostician to differentiate them.

It becomes necessary, in order that we shall not do injustice to the experimenters who have claimed to produce genuine cow-pox by the inoculation of cows with small-pox virus, to go over the history of these attempts as briefly as possible.

The impulse given to these attempts was by Jenner's theory, that vaccine was but a species of small-pox. His nomenclature fully proves this : thus, he names the vaccine disease *variola vaccinae*. Moreover, he fully enunciates his belief in the identity of small-pox and cow-pox, and his biographer says : * “ He always considered small-pox and cow-pox as modifications of the same distemper ; and that in employing vaccine lymph, we only made use of means to impregnate the constitution with the disease in its mildest, instead of propagating it in its virulent and contagious form.” † There has been always a traditional opinion of the identity of cow-pox, founded upon the statements of writers, contemporaries of Jenner, or following closely after him. There was little originality in any of the volumes advocating this theory, many of them following Jenner's narrative precisely, adding only some cases coming under their observation.

None of the variolous inoculations of cattle for artificial cow-pox claimed to be successful ‡ until 1807, nine years after Jenner gave his “ Inquiry ” to the world,—the first attempt reported as being successful, being performed by Dr. Gassner, of Günzberg. The results were called in question, says Trousseau.§ Twenty-nine years later, Thielé, of Kasan, in Russia, announced successful production of artificial cow-pox. The small-pox matter em-

* Baron's Life of Jenner, p. 241.

† The theory of the transmission of grease from the horse to the cow was the favorite theory of Jenner. Inquiry, pp. 58 and 59.

‡ These inoculations had been tried in vain by Coleman, Ring, Sacco, Numann, Fiard, Bousquet, Daltol, etc. Steinbrenner. Traite Sur la Vaccine, p. 613.

§ Lectures on Clinical Medicine. Vol. II. English Edition, p. 115.

ployed in these inoculations was taken immediately from variolous pustules, while they were yet transparent, nacreous, pearly, and of which the lymph was still very limpid; or, better still, he used lymph which had been kept ten to twelve days between two pieces of glass.* Steinbrenner, in remarking on the results of Thielé, says: "Let us say furthermore, *en passant*, that this author, convinced by his experience of the analogy of the vaccine and variolous virus, had the idea of trying to mitigate the latter, in order to make it produce the same results as vaccine, and that without making it pass previously through the body of the cow. He pretends it will become so by leaving variolous virus for ten or twelve days between two glasses closed with wax; then he only dilutes it with tepid milk, and uses it thus prepared. He repeated the same thing with new lymph for many [series] following. The secondary fever did not show itself; the virus becomes so mitigated in the sixth generation that it produces nothing more than the symptoms of ordinary vaccine. It can then be employed without further precautions to vaccinate from arm to arm." (Steinbrenner, p. 615.)

It is very evident from the above statement that if Thielé's observations as to variolous inoculation are no sounder than his opinion of extracting the virulency from small-pox virus, that we must be exceedingly guarded how we accept his opinion. In the light of our present knowledge, it would be the height of folly to dare to go before the public with such an announcement. Small-pox virus cannot be tamed by such gentle means; you may destroy it utterly, but so long as there is any activity left it maintains its same contagious properties, and probably cannot be so diluted as to be harmless. It is true, there are degrees of small-pox. Some types are so mild as to appear harmless. I inoculated one patient with small-pox virus in 1865, and he had only two resulting pustules, although there was no evidence of vaccination, judging by absence of cicatrices. But can any one deny that an unprotected person coming in contact with the mildest cases of small-pox; one like this, for instance, would not be liable to small-pox in some form?

* Steinbrenner, p. 614.

Steinbrenner himself also failed to inoculate two cattle upon which he made the attempt, although he says that he was not then informed of the minutæ of the process as detailed by Thielé and Ceely.

Dr. Ceely's cases are not so easily disposed of by merely denying his success. Dr. George Gregory * was willing to admit Ceely's success. "No doubt," he says, "can be entertained of their correctness." * * * "But "the lymph thus obtained has been called variolo-vaccine, to distinguish it from the idiopathic affection of the animal." "One effect of these experiments (of Ceely's) has been to refute Jenner's favorite notion, that the cow-pox is the parent of small-pox. So far as they go, they tend to show that small-pox is the primary, and cow-pox the secondary disorder. But it may be reasonably asked, do these experiments warrant the conclusion that cow-pox and small-pox are identical? To me it appears that they do not. The disorders are allied (so are measles and scarlatina), but they are not therefore identical.

* * * Unlike small-pox, cow-pox produces no eruption, no constitutional disturbance; it throws off no contagious emanations. It can be perpetuated from man to man in a uniform state of intensity; whereas the inoculation of small-pox produces the disorder in varying shades of severity. The local characters of each malady are no less strikingly contrasted. The variolous action goes on to pustulation, to the *acumination* of the pustule, to sloughing of the corion, and implication of the subjacent cellular membrane. The vesicle of cow-pox never loses its umbilicated character; no purulent matter forms; the areola is circular, not irregular, like that of the inoculated small-pox."

The above quotations are made from this well-known master, who for years was physician to the Highgate Small-pox Hospital, and who was a teacher of recognized ability at St. Thomas' Hospital. These opinions were expressed only a few years after Ceely's famous experiments were published.

When we consider these inoculations in the light of our present knowledge of the natural history of cow-pox as observed in the large number of vaccinations directly from the cow, it is plainly

* Gregory, Lectures on "*The Eruptive Fevers*," Am. Edition, 1851, p. 265.

evident that the matter obtained by Ceely was quite different from the animal virus of the Beaugency stock, now so well known in the United States.

It will be observed that writers upon the use of vaccine directly from the cow, especially as to the use of variolo-vaccine, give warning that a more violent form of vesicle would result from the insertion of such lymph; or, they instruct the vaccinator that frequent failures might be expected.

Dr. Seaton says (Handbook of Vaccination, p. 87): "Ceely found that more than half his attempts to vaccinate with primary cow-lymph, taken from vesicles at a proper stage, and possessing all the characteristics of perfection, resulted in entire failure, the same individuals being successfully vaccinated immediately afterward with the current humanized vaccine lymph."

With our knowledge of the course of a vaccination from the current Beaugency stock of animal virus, it is not difficult to believe that this virus which behaved so uncertainly had little relationship to the certain, normal, active stock now propagated in this country. All along through the history of the Ceely virus we read of uncertainties, of abnormal actions, and of suspicious phenomena, which virtually rendered its use impracticable, and which gave little encouragement for others to follow, except as a *dernier ressort*.

A new chapter on the production of artificial cow-pox was worked out during the late war, in the Confederate States. The question of the supply of genuine vaccine became such a serious one, that medical officers were commissioned to undertake the study of the immense number of spurious vaccinations, and devise the remedy. I speak within bounds when I say that in this country it was rare to find a medical man who was at all skeptical as to the possibility of procuring artificial vaccination at will. The experiments of Ceely were studied with peculiar interest, as were those of Sonderland. After attempting again and again to restore the efficacy of vaccine by careful cultivation among country children, no adequate supply was obtained, and then the variolation of cattle, by the Ceely and Sonderland method, was resorted to. Dr. James Bolton, late of Richmond, Va., undertook the experiments for the production of artificial

cow-pox. He says: * “I made experiments upon seven animals during the war. Five of them were young cows, or heifers, which were furnished me by the Commissary Department, by order of the Surgeon-General. They were kept at Howard’s Grove, in the immediate vicinity of the Small-Pox Hospital. From the latter place I procured variolous virus in the form of lymph and crust. This I inserted into the udder by making light incisions, and rubbing into them while in the liquid state, by dipping threads into the lymph and passing them by means of a needle under the integument; and finally by making punctures with a lancet and introducing pieces of crusts; but all these attempts failed. In two instances pustules were produced, followed by thin yellow crusts, which were evidently spurious results.” * * * “I have my doubts that genuine vaccine is obtained by inoculating cows with small-pox virus.” Subsequently Dr. Bolton wrote me: “I have my doubts that the small-pox virus can be inserted at any part of the udder from which it cannot be licked off. The tongue of the cow can be protruded so far, and can be drawn into such anomalous curves, with such prehensile power, that I think any part of the udder can be reached.” The details of Dr. Bolton’s experiments were destroyed by the casualties of war, and his account was written from memory.

Dr. S. P. Crawford, of Greenville, Tenn., writes to Prof. Joseph Jones, M.D.: † “About this time” (January, 1865) “orders were issued from Surgeon-General Moore to procure fresh vaccine virus through the cow. I tried the experiment three times, without succeeding. First on a cow seven years old, with a calf six months old; then a young heifer two years old, and finally the calf. I inserted the matter in the teats, nose, ears, and various parts of the skin, but never succeeded in getting a crust. Finally I fed the calf on the dried scabs, but it died without yielding the much sought treasure.”

Dr. William J. Love, of Wilmington, ‡ “had occasion to at-

* Non-Identity of Vaccinia and Variola, by Thomas F. Wood, M.D., 1867, p. 17.

† Researches Upon “Spurious Vaccination” During the American Civil War. By Joseph Jones, M.D. Nashville, 1867, p. 99.

‡ Non-Identity of Vaccinia and Variola, p. 17.

tempt the same experiment" [variolous inoculation of a cow], "and failed." He introduced the small-pox virus into the teats and udder, by means of a saturated thread. He also made incisions a little deeper than in the human subject for like purpose, and introduced the lymph in this way. Finally he smeared collard leaves with the lymph, and fed the cow on them. He saw no other phenomena, except such as would result from the insertion of any foreign matter,

I had occasion just after the war (1865-'66), while in charge of the Wilmington Small-Pox Hospital during an epidemic of the disease, to go over the same ground of attempting the production of artificial cow-pox. My experiments were done without any knowledge on my part of the details of the operation, except such as I had received through the gentlemen above quoted, and by the perusals of Ceely, Thielé's and Sonderland's experiments. It is not necessary to detail these experiments, as they differed in no respect, as far as I was able to judge, from the description given by these authors. The failure was complete, in my opinion.

It happened, though, that during the progress of the experiment, that an army medical inspector, whose name I have forgotten, was making a tour of the hospitals, hearing of my experiments, visited my hospital, and after examination pronounced the small vesicles genuine cow-pox, and confirmed his faith in his opinion by making some inoculations on the arms of two children in an Irish family near by. The inoculation resulted in a genuine small-pox, which went through the family in various grades of intensity.

A reviewer in the *Medical Times and Gazette*, July 2, 1881, discussing Mr. Fleming's recent work, "Human and Animal Variolæ," says: "It seems to be commonly held among medical men in this country that cow-pox is not a disease natural to the bovine species, but merely small-pox conveyed to them from man by inoculation, and that in the operation of vaccination we avail ourselves of the modification which it has undergone in the body of the cow, precisely as Dr. Greenfield confers immunity from anthrax on cattle by inoculation with bacilli which have passed through the system of the rabbit.

"In support of this view, it is usual to adduce the supposed

rarity of cow-pox now that small-pox has become almost unknown in rural districts, the asserted exemption of the male sex among cattle, the successful inoculation of cattle with small-pox, with the production of what Thielé and Ceely believe to be cow-pox, and the vaccination of infants therefrom, by the same observers.

“Mr. Fleming has collected a vast amount of evidence from French, German, Scandinavian and Italian sources, and has carefully scrutinized the scanty English experimenters. He gives irresistible evidence of the prevalence of cow-pox in countries where small-pox is almost unknown,* of the equal susceptibility of bulls, if exposed to the contagion with cows, and satisfactorily proves that the supposed vaccinations of Mr. Ceely were, in fact, nothing more or less than an inoculation with small-pox unchanged by its transmission through the cow. Such inoculation of the cow is not often successful, but in several instances the real nature of the lymph thence taken and implanted in children has been manifested not only by the eruption following, but by the communication of unmistakable and fatal small-pox to other infants by ordinary infection—a caution to those who would attempt to reproduce a more active vaccine through such a procedure as mediate variolation. * * * The disastrous results of mediate variolation instead of vaccination, in America and India, show the value of a knowledge of the diseases of domestic animals.”

The *opinions* as to the non-identity of vaccinia and variola are very numerous, and writers are about equally divided, perhaps. It possibly may not be definitively settled in our day, and indeed, the question of identity may be no longer a practical one. This will depend greatly upon the way in which the medical profession makes use of the triumphant solution of the question as established in the introduction of animal virus by Dr. Henry A. Martin. It does not seem possible that with our present enlightenment, a vaccine famine can occur in this country, and for years no more attempts will be made to produce artificial cow-pox. Never-

* Cow-pox has appeared in communities where there has been no small-pox at the time. A recent most convincing series of cases were reported by Dr. Henry A. Martin, of Boston, at the last meeting of the American Medical Association. These cases of spontaneous cow-pox occurred in 1880, in Massachusetts, and their relation was rather to horse-pox than small-pox, a case of the former having also occurred in the same neighborhood.

theless, the warning should go forth authoritatively, that the production of artificial vaccination is a myth, and in the times of great danger from variolous epidemic, the time when it is most eagerly desired to do something effectual, it has most signally failed.

Let the decision be what it may as to the theory of identity, it is practically valueless, and always dangerous, both on account of the delay, and of the risk of pure variolous inoculation. Those who have claimed success have left such a record of difficulties to overcome, and disappointments to suffer, that we can no longer have faith in it as a source of vaccine, even though we should not discredit the truth of identity.

I had intended to discuss more fully the points in Dr. Lyman's paper on vaccination, above referred to, but I have overstepped the bounds in consideration, mainly, of the question of identity. I am pleased to see that we agree as to efficacy of animal virus. I have nothing but praise for the results of vaccination done with Beaugency virus. I trust that the potency of this virus may forever prevent the adoption of Dr. Lyman's suggestion: "The only efficient remedy for this deterioration [referring to the possibility of the degeneracy of bovine virus] consists in occasional reversions to the original product of the variolated cow." I am convinced that more diligent searching by experts will detect now and again genuine cow-pox. From this source alone will the pure succession be maintained. Knowledge is accumulating very greatly upon the natural history of cow-pox, and a widespread interest will surely manifest itself among intelligent farmers, making the mishap of the non-occurrence of cow-pox less and less probable.

ARTICLE IV.

ECLAMPSIA, OR PUERPERAL CONVULSIONS. By D. A. WALDEN, M.D., Beatrice, Neb.

In May, 1879, I was called to see Mrs. H., a primipara, age 23 years, in her seventh month of gestation. Found her in puerperal convulsions, which would last from one to five minutes, and each paroxysm was followed almost immediately by another.

I remained with the patient all night, and administered chloroform by inhalation, but could not control the convulsions. In the morning I began to administer morphia sulph. in one gr. doses, but with no better results. I followed this with hydrate chloral, 60 grains every two hours, and bromide potass., 80 grains every three hours, and continued this treatment for forty-eight hours, without controlling the convulsions in the least.

Counsel was called, and a delivery was concluded to be the only means of saving life. Fœtus was dead—woman made perfect recovery.

In March of the year 1881 the same woman was taken, in her eighth month of pregnancy, with puerperal convulsions the second time. I was called to see her, and found her almost as before; gave her the same remedial treatment as before, but without any good results.

The idea occurred to me: would not quinia have good effect in such cases? I concluded to try it, at least, as I had tried all remedies that I could find in books on that disease. I wrote a prescription for quinia sulph., grs. 80, to be divided into three powders, one to be given every two hours.

After an hour from the time the first powder was taken I could see a change; the convulsions were becoming lighter. Continued the quinia until six 10 gr. powders were taken, at which time the convulsions were entirely controlled, and did not again return.

The patient went to her full term, and then I delivered her of a healthy female child. The mother and child are doing well up to this date.

Case second. Mrs. B., a multipara, age 24 years, in her eighth month of pregnancy, was taken with puerperal convulsions on July 8, 1881. I was called to see her; found her in convulsions most of the time; remained with her all night, and gave chloroform by inhalation without any effect. Then remembering my former case, I ordered quinia sulph., grs. 60, divided into six powders, one to be taken every two hours. A change was noticed in about an hour after the first dose was taken, and the convulsions had ceased entirely by the time the six doses were taken, but came on again the next day. I then ordered the quinine again, and continued it for three days.

The convulsions did not again appear, the patient went to her full term, and I then delivered her of a healthy male child.

Case third did not come directly under my notice, yet I will cite it, to further prove that there is really some virtue in quinia for this disease.

After trying quinia in my first case, I related my experience to Dr. S. G. Huff, of this place, and soon after, he, having a case of the same kind, thought, after trying the usual remedies of bromide potass., chloral hydrate, chloroform, etc., that he would give the quinia treatment a trial, as the more that bromide potass., chloral hydrate, etc., were given, the worse the patient became. The convulsions were so severe that the assistance of two or three persons was required to keep the patient in bed.

He put her on 10 gr. doses of quinia, one to be given every two hours; it controlled the convulsions completely, and she has not had a return of them since. Her entire term of gestation is now nearly up.

I will not enter into the pathological condition of puerperal convulsions, for that has been fully discussed by all of our eminent authors, yet I would say to the profession that it may be of some interest and benefit to give quinine a fair trial in this disease.

ARTICLE V.

STUDIES IN THE MEDICAL BOTANY OF SOUTHERN ILLINOIS.

By J. M. G. CARTER, A.M., M.D.

The study of botany is exceedingly interesting, not only on account of the pleasure derived from considering the endless varieties of plant, flower and fruit, but on account of the healthful exercise and growth of mind connected with its observations, investigations and classifications. The study becomes doubly interesting when in addition to the foregoing we take into consideration, as the physician does, the remedial value of the active principles of plants in disease. Whatever is life-saving or health-giving has a peculiar charm about it, and when we consider that the vegetable kingdom furnishes us most of these desirable agents, we have probably the strongest incentive to its study which could

draw the mind to the consideration and investigation of any science known to man. Every physician ought to be acquainted with the science of botany so far as it pertains to medicine, and especially the medical botany of his own neighborhood. Not unfrequently he is called to a case (particularly is this so in country practice), where a medicine is required which he does not happen to have with him, but which may be growing in the woods near by. An acquaintance with this branch of medicine at such a time as this can easily be appreciated.

I do not pretend to say that there is a relation between the diseases of a country and its flora, nor venture an opinion as to what that relation, if there is one, may be; but I believe it is an established fact, that where a particular disease seems to be indigenous, *there* is found in the vegetable kingdom a remedy for its cure, and very frequently the best remedy. In this place I might refer to malaria in all its forms, so tenacious in this part of the State, to relieve which we have wahoo, dogwood, mandrake, etc. The same disease prevails in South America, and no remedy has a wider reputation than cinchona, of the same country.

A large proportion of all the medicinal plants indigenous to the United States can be found in Southern Illinois; but those that are used especially in the treatment of diseases of this climate are most abundant.

In this paper I can only refer to the families of plants and some of the more important individuals; those not supposed to be well known, and those having some interesting remedial application in domestic practice among the people of this portion of the State. For descriptions of these plants, I must refer the reader to Gray's Manual, or any other good manual of the botany of Illinois.

The crowfoot family (Ranunculaceæ), the order to which aconite and coptis belong, is represented in Southern Illinois by a score or two of medicinal plants, the most important of which are *Hydrastis Canadensis* and *Cimicifuga Racemosa*. Yellow puccoon root is frequently used in decoction or infusion for "chills," jaundice and debility, with excellent effect. It is also used with benefit as a mouth wash in various forms of stomatitis. I use the powdered root with chlorate of potassa or alum. in

ulcerated sore mouth, and I have often found the combination to be superior to the potash alone. The people not uncommonly use an infusion of the root as a wash for ulcers. Cohosh, or blacksnake root, is often used in rheumatism, dropsy and fevers.

The magnolia family (Magnoliaceæ) has but one representative so far as I know. This is the *Liriodendron Tulipifera*. It is a magnificent tree, generally called poplar. The bark of any part, especially the root, may be used in infusion or powder as a tonic, and in malarial fevers, more particularly intermittent. The ordinary strength of the infusion is 3j to Oj of water. The powder may be given in doses of 30 to 60 grains.

Podophyllum Peltatum is the only plant of the barberry family (Berberidaceæ) which deserves mention in this paper. This plant, known as the May apple, or mandrake, is largely used in this portion of the State in bilious fevers (remittent and intermittent); it is sometimes combined with calomel and sometimes given alone, or in combination with other vegetable cathartics. In these fevers, when it is necessary to keep up cathartic action for several days, I frequently give two or three full doses of calomel, and follow with *podophyllum* and blue mass. In splenic dropsy mandrake in connection with tonic doses of quinine is a most valuable remedy.

Poppy family (Papaveraceæ). I am not aware of any indigenous variety of the poppy in Southern Illinois, but the *Papaver Somniferum* is occasionally cultivated and used in domestic practice, in infusion or decoction, as an anodyne to sores, and sometimes internally. It does not seem to be as strong as the eastern or continental plant; perhaps because not so much care is bestowed upon its cultivation here as in the old world. I know of no experiments with the poppy produced here to determine its value; but presume its value is equivalent to that of any other portion of the United States. Opium, in its various forms, is very abundantly employed in both general and domestic practice, in the malarial diseases of this section. However, there is quite a difference of opinion in regard to the relative value of the various preparations. Some use the pulverized opium, many Dover's powders, and many more morphine. The great drawback to the employment of any of these through the stomach, is their consti-

pating effect, as it is considered by nearly all physicians in this country that the bowels should be opened immediately in bilious fevers. To avoid the constipating tendency of opium, some prescribe svapnia, others use morphine hypodermically. My experience with the hypodermic injection of morphia has not been extensive enough to make me feel safe in making a positive statement that so used it does not constipate; still I have thought that I could see that it was not likely to do so, and many physicians are very confident in their assertions that used in this way, morphia does not affect the bowels.

Sanguinaria Canadensis, another plant of the poppy family, is very abundant in this part of the State, being known by the name of *red puccoon root*. Blood-root is used in general and domestic practice as an alterative and tonic chiefly, in amenorrhœa and inactive liver, in cases of chronic malaria, etc. It may be used in combination with Podophyllum Peltatum in splenic dropsy. It is used by the laity principally as "bitters," the dried root in some form of spirituous liquor—usually whisky. Occasionally it is so used in combination with Hydrastis Canadensis. It must be used in small doses, and continued for considerable time. I had a case of acute jaundice last January, due to obstruction of the gall duct—an effect of malaria. The case became chronic, and I used sanguinaria for two months, with ultimate recovery.

The mustard family (Cruciferae) is another order with a large representation in our section, but I shall mention only three, the others not being of any great importance. Nasturtium Armoracia (horse-radish), Sinapis Nigra (black mustard), and Sinapis Alba (white mustard), are very important plants, and the first two especially are used in domestic practice as emetics and counter-irritants. Not unfrequently, when I have not been able to secure mustard, I have had horse-radish scraped, and used as a poultice to produce counter-irritation in bilious pneumonia, or "winter fever." The results have been good, and I have no doubt that it might be safely used as a substitute for mustard in any case where mustard is indicated, provided care be taken. The mustards (black and white) do not seem to be indigenous, but were first brought from Europe, and have now escaped from

gardens. Horse-radish is native in this State. Treacle mustard (*Erysimum*), and hedge mustard (*Sisymbrium*) may be used as substitutes.

Besides prickly-ash (*Zanthoxylum*), the rue family (*Rutaceæ*) contains wafer-ash, or hop-tree (*Ptelea Trifoliata*). Wafer-ash is an excellent tonic to the stomach and intestines, especially in chronic malarial and typhoid cases. Combined with pepsin, it is greatly beneficial in dyspepsia. I have used it with good results, thus combined, in cases where sometimes "heartburn" and sometimes distressing pain is felt after eating. I have often given it four or five hours after a meal, when the stomach is disturbed and painful, and have seldom known it to fail to give immediate relief. An excellent preparation is manufactured by Chapman & Green, of Chicago.

The remedy may be used in infusion, or better, in tincture prepared, according to Dr. Potter, by the following formula :

℞.	Hoptree bark.....	℥ vi.
	Ginger....	℥ ss.
	Whisky.....	O iv.

M. Sig. Tablespoonful thrice daily.

The Cashew family (*Anacardiaceæ*) also contains some plants of considerable remedial value. In a country where so much calomel is used as in this, and where at least half the people have been salivated, it is of great service to have a remedy specially adapted to cure such cases of sore mouth. Sumach (*Rhus Glabrum*) has a high reputation in this malady. It has also been recommended as a cooling drink in febrile complaints. An infusion of the berries is used. It is somewhat astringent.

Poison ivy, or poison oak (*Rhus Toxicodendron*), concerning which so much has been said and written within the past few years, belongs to this family, and is very abundant in Southern Illinois. The poison vine (*R. Radicans*) is also abundant. The last two plants, and some others of the same family, are not used, so far as I know, in domestic practice, and are only feared by the people for their poisonous properties.

The principal representative of the staff-tree family is wahoo (*Euonymus*), or burning-bush, sometimes called Indian Arrow. The variety I speak of is *Euonymus Atropurpureus*. I have already mentioned this as an important remedy in malaria.

This plant, or shrub (Nat. Order Celastraceæ), is found in low grounds and swamps. The bark is the part used, an infusion of which is often taken as a tonic in chills and fever. It has been used with success in dropsies. In its cathartic action it somewhat resembles rhubarb. It is a diuretic, and supposed to be an alterative, acting especially on the liver. It is used in infusion in splenic dropsy.

This large rose family (Rosaceæ) is well represented in our woods. The wild cherry (*Prunus Virginiana*, *P. Scrotina*) belongs to this family, and is abundantly used as a tonic with iron, quinine and wahoo. Its tonic value is seen in general debility, malaria, and hectic fever of consumption. To this family belong also strawberry (*Fragaria Illinoiensis*), raspberry (*Rubus Odoratus*, *R. Strigosus*, *R. Occidentalis*), blackberry (*Rubus Villosus*), and dewberry (*Rubus Canadensis*). The root of all these may be used similarly to the blackberry of the U. S. Dispensatory.

The saxifrage family (Saxifragaceæ), in addition to several other medicinal plants, is represented in alum-root (*Heuchera Americana*). This root is used where other astringents would be indicated, and at one time had quite a reputation as a cancer medicine. It may be used in powder, decoction or infusion.

Passing by Hamamelis and Liquidambar, of witch-hazel family, *Oenothera*, of primrose family, all of which are valuable remedial agents, I mention the parsley family (Umbelliferæ), chiefly because it contains *Conium Maculatum*, a most excellent substitute for opium when it is desired to avoid constipation. This plant is said to grow in Southern Illinois; however, I have not seen it. *Deucus Carota*, common carrot, belongs to the same family. An infusion of the root is an excellent nephritic remedy in chronic cases. It is used in dropsy. Water hemlock (*Cicuta*), parsnip (*Pastinacea*), and other indigenous medicinal plants belong to the same family.

To the ginseng family (Araliaceæ) belong false sarsaparilla (*Aralia Nudicaulis*) and ginseng (*Panax Quinquefolium*). The root of the latter is used in whisky, by some people in this part of the State, as a general tonic. This plant is valued very highly by the Chinese, as a remedy in all diseases.

The dogwood family (Cornaceæ) is another family useful in medicine. Dogwood (*Cornus Florida*) is an excellent substitute for quinine. It is not unfrequently used as a tonic, in the form of a tea or bitters made of the bark. When quinine has failed in intermittents, dogwood often will effect a cure. Other varieties of dogwood (*C. Cincinata*, *C. Sericea*, *C. Asperifolia*), may be used as substitutes for *Cornus Florida*.

Only fifteen of the thirty-one orders, or families, of polypetalous exogenous plants, found in Southern Illinois, that contain medicinal plants, have been mentioned; and these thirty-one orders do not contain forty per cent. of the medicinal plants indigenous to this portion of the State. The fifteen orders mentioned contain the most important remedies of this botanical division. The remaining botanical divisions are left for future study.

This outline of the medicinal plants in only fifteen families is sufficient to show that our State is rich in medicinal agents. One hundred plants might have been given, instead of less than forty; but perhaps the list has been sufficiently extended.

ARTICLE IV.

THE PATHOLOGY AND TREATMENT OF YELLOW FEVER; WITH SOME REMARKS UPON THE NATURE OF ITS CAUSE AND ITS PREVENTION. By H. D. Schmidt, M.D., New Orleans, La. (Continued from page 312, September No., 1881.)

In drawing a final conclusion from what has been said in the preceding pages upon the bacteria question, we may well presume that they do not represent the direct cause of those diseases with which they have been found associated, unless stronger and more reliable proofs are brought forward in favor of this theory. But, as by this time, in most diseases, the blood and tissues have been examined over and over again by numerous observers, who are anxious, besides, to "find something," without the discovery of other facts than those known for a number of years, it appears quite improbable that this will ever occur.

In order to show that all statements, regarding the presence of minute organisms in the blood and tissues of individuals affected

by infectious diseases, must be received with great caution, I shall, before dismissing the theory of the "contagium vivum" in general, cite the views of a few of the most pronounced germ-theorists on the subject. Thus, *Liebermeister*, who theorizes even upon invisible germs, says, more than six years ago:* "It is true that the triumph of the theory of a contagium vivum is in no way complete, and as in former times, so now, it is not so much its opponents, as it is the imprudent adherents who threaten to bring the theory into discredit. The utter lack of critical discernment and method which have characterized some of the works in this field, and, on the other hand, the recklessness with which facts of uncertain significance have been proclaimed certain proofs, have also in our time driven away many earnest investigators."

Koch,† the discoverer of the spores of *Bacillus anthracis*, after testifying that "in the blood and tissues of the healthy animal or human organism bacteria are not met with," continues as follows: "It appears to me, however, that the following objections against the acceptance of bacteria being the cause of infectious traumatic diseases are justified. To gain an incontestable proof for this acceptance it is essential *that the bacteria, without exception and in proportions regarding their number and distribution, should be shown in such a manner, as to perfectly explain the symptoms of the respective disease.* For, if in some cases of a certain kind of infectious traumatic disease bacteria are found, while they are absent in other cases exactly alike,—and if, furthermore, the bacteria are present in too small a number as possibly to cause a severe disease or even a fatal issue, it becomes self-evident that their inconstant appearance is dependent upon chance, and their small number no sufficient proof for regarding them as the sole cause of the respective disease, and that we must accept the existence of other causes besides. The observations made in regard to the presence of bacteria in infectious traumatic diseases, however, do in fact not correspond with the demands required for an incontestable proof."

* Cyclopædia of the Practice of Medicine, edited by Dr. H. v. Ziemssen; American edition, Vol. I.

† Koch, R.—*Untersuchungen ueber die Ätiologie der Wundinfections-krankheiten.* Leipzig, 1878, p. 22.

Birch-Hirschfeld,* in speaking of the pathological signification of bacteria makes the following remarks: "But it must be admitted that, even if the constant occurrence of these organisms in the diseases mentioned is recognized as a fact, doubts may yet arise as to whether these bacteria are the real cause of the disease."

"This question will not arise with an infectious disease in which organisms of a certain morphological character constantly and exclusively occur, as in recurrent fever, where the spirilla claim a diagnostic signification. In splenic fever this circumstance is less significant."

"The morphological character of the bacteria found in pyæmia, diphtheritis, small-pox, cholera, however, are so much the same, as to give rise to the idea that they are identical forms. But the conclusion drawn from this would be that no specific signification could be assigned to these organisms. Accordingly, they would not represent the cause but parasites of the disease, which, even then, may not be without signification, or may stand in some relationship with certain septic conditions of some infectious diseases."

After the preceding discussion of the so-called "bacteria theory" and of those diseases in which these minute organisms are really met with in the blood and tissues of many cases, we are now ready to inquire *whether this theory may in any way be applicable to yellow fever*; and, in order to arrive at correct conclusions from such an inquiry, it becomes essential that we should set aside all postulation, such as the existence of "invisible" living germs, and strictly keep to observed positive or negative facts. It is self-evident, therefore, that the assertion of bacteria or their spores being the cause of yellow fever must be substantiated by the ocular demonstrations of these organisms in the fresh blood of the patient, before it can be received as a truth. Judging from my own numerous examinations of yellow fever blood under various conditions, already mentioned in this treatise, I need not hesitate to say that this has been done. And, since the results which I repeatedly obtained ever since the yellow fever epidemic of 1867, have now been corroborated by those

* *Birch-Hirschfeld*.—*Lehrbuch der Pathologischen Anatomie*, p. 233.

of *Sternberg*, who, during 1879, examined quite a number of specimens of fresh yellow fever blood at Havana, I may add that all statements to the contrary must have been founded upon an imperfect and inexact manner of microscopical examination. And as, furthermore, bacteria are neither met with in the tissues and organs of fatal cases of yellow fever after death, if properly prepared while still in a fresh condition, we are justified in presuming without further extending the argument, *that yellow fever does not depend upon the presence of bacteria or any other minute living organisms in the blood or tissues*, for, if it did, *the microscope would most certainly show them*, as well as it has done in those other particular diseases mentioned.

But, it will now be asked, if the cause of yellow fever is not represented by minute living organisms, what is it then? And there are many persons, both physicians and laymen, who will promptly answer this question by referring the cause to the products of vegetable and animal decomposition in the form of filth and dirt in the streets and houses. But before I shall attempt to show that the "filth-theory" has no better foundation than the "living-germ-theory," it becomes necessary that I should properly define the standpoint from which I view the subject, as otherwise I might be looked upon as an advocate of dirt and filth. There is hardly a physician who does not consider a supply of fresh and pure air and water as the cardinal objects in public and private hygiene; nor have I, myself, ever regarded these elements in any other light. On the contrary, the degree of civilization of a community may rightly be judged from the degree of neatness and cleanliness of the houses and streets; and nobody will doubt but that the hygienic condition of a community depends to a considerable extent upon the purity of the air breathed by the people in their dwelling-places. It is also true that some local affections may arise from the breathing of air contaminated by gases arising from privies, or from other animal and vegetable decomposition. Such cases may occur in any large city, though not very often; but, that a *specific* and *contagious* disease, characterized by *specific symptoms and pathological changes in the tissues and organs*, should take its origin from such a cause is contrary to all modern scientific knowledge. A

specific disease can only be caused by a specific poison ; and though certain pathological processes may equally take place in different specific diseases, yet each of the latter maintains its own specific character besides. The poison of yellow fever will never produce scarlatina, and the poison of the latter will never give rise to yellow fever, though in both diseases parenchymatous degeneration of the kidneys may be met with. The development of a specific disease may be compared with that of the egg ; for a certain mysterious agency—similar to that determining the specific characters of an animal or plant, and lying dormant in the protoplasm of the egg-cell—also resides in the poison of a specific disease, determining the specificity of its symptoms and pathological changes. And, accordingly, the inquiry into the origin of these specific poisons will be as resultless as that into the origin of the first egg. As far as I am able to judge, it appears to me that specific diseases have never sprung suddenly into existence, but that their origin must be compared to that of vegetable and animal species, and that, throughout an immense length of time, they have been developed into what they are now by the processes of “selection” and “adaptation.” Thus, small-pox, or yellow fever, may have presented a different character thousands of years ago, from what they do at present.

Yellow fever, therefore, being a specific disease, cannot arise spontaneously through the influence of the air rendered impure by the products of vegetable and animal decomposition, for, if this were true, New Orleans would be depopulated long ago. From all I know, the disease, in this city, has never arisen at the outskirts, or in the vicinity of those places to which the kitchen refuse and rubbish of the houses used to be carried not many years ago, or of grave-yards, such as the Locust Grove Cemetery of bad repute, but, on the contrary, always in the populated commercial centers, or in the vicinity of the harbor. Besides, it is a historical fact that this disease almost always first appears in a sea-port, before it extends into the interior of the country, and that it stands in close relationship with the shipping over sea. If yellow fever could ever have originated from the gases arising from decomposing matter, it should have certainly done so in the vicinity of one of those so-called dumping grounds, a large empty

lot, situated, not many years ago, on Broad Street Canal between St. Anne and Dumaine Street, and which, in the pursuit of my practice, I had frequent occasion to pass. Whenever I passed, I found it occupied by quite a number of negro rag-pickers of both sexes, accompanied by their dogs, and engaged in digging the ground in search of lost treasures. The stench arising from this place was so offensive and sickening in character that I had to rouse my horse and hold my breath until I had passed the whole square. In the same way I have frequently met with offensive odors, though differing in character from that of the garbage, arising from the stagnant water of the canal itself, yet I never heard of yellow fever originating in that vicinity, nor did I learn that other diseases prevailed to a greater extent there than elsewhere. As regards the spontaneous origin of diseases in the vicinity of the grave-yards in the city, I need only point to the report of a committee, appointed by the Medical and Surgical Association of New Orleans, for the investigation of this matter, and which answered the question in the negative. Many other instances might be adduced to show that no relation exists between the origin of yellow fever and the filthy condition of some of the streets of New Orleans, or any other city. With this assertion, however, I by no means advocate the existence of grave-yards in the heart of the city, or bad drainage and muddy and filthy streets and houses, for there remains no doubt but that whenever the disease has once been introduced and assumed the character of an epidemic, nothing will contribute more to it being fostered and propagated than filth and insufficient ventilation. This I know from experience.

It has also been asserted that yellow fever was caused by gases arising from the bilge-water in the hold of the ships, and the latter has even been regarded by some germ theorists as the medium by which the "contagium vivum" was transmitted from port to port. As regards the gases emitted from foul bilge-water, the remarks made on filth and dirt will be equally applicable to this liquid; but, as concerns the living germs which it is supposed to contain, the supporters of such a theory seem to have forgotten that these minute organisms are all inhabitants of the water or other liquids, which they are incapable of leaving except

by evaporation; for, minute as they are, they are still too heavy to be lifted out of the liquid with the molecules constituting its vapors, while in a dry condition they may be carried about by the slightest current of air.

Besides the reasons I have given above for showing the futility of the theory of yellow fever originating in filth, or from the gaseous products of decomposing matter, I might still further enlarge upon this subject,—a subject which, by having been treated so often and so extensively, both in medical and public literature, is now too well known to require any further comment on my part. Nevertheless, in support of my own views, I cannot forbear to cite the views of two distinguished germ-theorists before dismissing it. Thus, Liebermeister* says: “Not a very long time ago it was almost universally accepted that merely the coincidence of certain especial conditions were necessary to cause the autochthonous appearance of certain infectious diseases. As early as the time of the Athenian plague, Diodorus found a sufficient explanation for the origin of the disease in the circumstance that a great multitude of people from all quarters streamed into the city, and, being cramped for room, breathed a corrupted air; “thus they were attacked with disease in an inexplicable manner.” How many thousand times since then have medical and non-medical writers pictured social squalor, decomposing filth, unfavorable weather, etc., as the cause of disease! And physicians and laymen were accustomed to take it for granted that the plague and similar severe epidemics originated after this fashion. Many physicians found no difficulty in explaining the origin of syphilis from the mingling and concentration of the immoral elements of numerous nations. The plague arose from the imperfect burial of human corpses, with the consequent corruption of the air; yellow fever originated from foul bilge-water or from crowding of men together in slave ships; typhus fever from the crowding together of persons in badly ventilated dwellings, or from hunger; the cholera from rotten or unripe vegetable food; the typhoid fever from the exhalations of putrid excrements, etc. For many it was an interesting spectacle to realize

* v. Ziemssen, “Cyclopædia of Practice of Medicine,” Vol. I, p. 17.

how the great regulatory operations of nature should be so simple and easily understood, and how every considerable deviation from a proper observation of hygienic laws should immediately be punished by the production of a particular disease. In recent times the standpoint has been essentially changed. The potency of the factors in the *extension* of the diseases in question is not questioned; on the contrary, our knowledge of it has become more reliable and exact. We have learned, however, that the diseases do not *originate* in this way. It was observed that the battle-fields of Inkermann, strewn with corpses, whose stench drove the armies away, produced no pestilence. We have been convinced that, in spite of the prophecies and premature results, the investment of Metz was never able to produce a single case of typhus fever, inside or outside of the city, or to transform typhoid fever, which prevails there so frequently, to a possibly higher potency, *i. e.*, to typhus fever. We have gradually reached the conclusion that it is only where the specific germ of the disease exists by itself, or has been introduced, that those anti-hygienic factors become active and may then be capable of occasioning an enormous extension of the disease. The germ, however, is not produced by spontaneous generation."

"In fact, at the present time the acceptance of an autochthonous origin for syphilis is, for most physicians, as great an absurdity even as the origin of scabies or of lumbricoid worms by spontaneous generation. It is unanimously said of the plague, cholera, and yellow fever, that they never arise spontaneously, at least on European soil. The spontaneous origin of small-pox, measles and scarlet fever could hardly find a defender now. Perhaps the time is not far distant, when the doctrine of the spontaneous origin of typhoid fever, dysentery, typhus fever, etc., will be universally rejected."

Naegeli, from whose work on "The Lower Fungi and Their Relation to Infectious Diseases," I have already quoted in this section, speaks of the influence of the air upon these diseases as follows: "The air, containing bad smelling or other gases, though disagreeable, is not rendered noxious by it, particularly with regard to infectious diseases. With it, however, the different forms of more or less dangerous cleft-fungi, suspended in the dust-like masses, may be introduced into the body."

“The infected air itself is odorless. A putrefying substance only becomes dangerous after it has become dry, and the bad odor has disappeared. The miasms and contagia are also imperceptible for the organ of smell.”

“The microscopical and chemical examination of the residue of filtered air teaches us nothing as regards its noxiousness, as the infectious matters cannot be distinguished. It will be impossible to pass a judgment upon the infectious nature of any air, before we are able to test the effects of this residue in an experimental manner.”

“It is impossible to render infected air innoxious, as it cannot be made free from dust. At the utmost, the dust suspended in the air of an inclosed space may be removed by the vapor of water, or by sprinkling the walls.”

By some the origin of yellow fever, as well as that of other diseases, has even been referred to our drinking water becoming pregnant with decomposing vegetable matter while standing in the over-ground cisterns. I shall once more draw from Naegeli's work and cite this author's opinion on this subject. He says: “The drinking water (from wells, rivers, ponds, lakes, ground-water), even if not clean, is not deleterious to health (provided poisons do not get accidentally into it), and, least of all, causes infectious diseases, as those substances rendering it unclean are generally harmless in their nature; the fungi of putrefaction and putrid substances, as well as miasma-fungi, which it contains, represent so small a quantity, as to reduce the probability of a contagion to an imperceptible minimum.”

“With this corresponds the experience, which shows that the continual eating of large quantities of humus substances and products of putrefaction, as are contained in foul drinking water, as well as the exclusive and constant use of the latter by whole populations, are without deleterious hygienic effects. A critical consideration of the known facts relating to the spread of infectious diseases, also, shows that contagion has been wrongly ascribed to the water.”

The idea that the air we breathe is pregnant with fungus spores and bacteria, and that hundreds of these minute organisms are inhaled with every inspiration, seems to prevail among a

number of medical men; and it is this coarse error which has rendered the germ-theory so acceptable to the medical practitioner. It is true that under certain conditions these minute organisms are met with in the air, though in a comparatively small number, for the reason that the natural media in which they live are fluids in a certain state of decomposition, in which they multiply to countless numbers. Their comparative scarcity in the air has been satisfactorily proved by the observations of *Cunningham*, made some years ago at Calcutta, for the purpose of determining whether there existed any definite relation between the quantity of organic matter in the air and the prevailing diseases, such as diarrhœa, dysentery, cholera, miasmatic fever and dengue, or whether these diseases depended upon certain forms of organisms suspended in the surrounding air. For this purpose he used a weather-vane in the form of a tube, containing a plate of glass covered with a layer of glycerine, to which all particles contained in the surrounding air would adhere. The apparatus was placed in the vicinity of a prison and five feet above the ground. The matter adhering to the glycerine consisted of particles of silica, amorphous granules, carbon, lime, starch granules, cells, hair, fragments of vegetable tissue, filaments of cotton, hairs of insects, oil globules, pollen granules, spores and cells of fungi and algae, sometimes also bacteria, but in a small number. It was impossible to show a relationship existing between the amount of these particles and the diseases above mentioned.

In New Orleans, where rain-water, collected from the roofs of the houses into over-ground cisterns, is used for drinking or cooking purposes, similar objects are frequently found in the water, especially after a long draught, when the dust floating in the air has had sufficient time to settle upon the roofs of the houses. But I have never heard that diseases have arisen from the drinking of the cistern-water containing these miscellaneous particles, except, perhaps, when the water had been standing for a long time in an old foul cistern,—and even then such an occurrence would be very improbable. On the contrary, our drinking water is the purest that could be anywhere obtained, especially after being filtered, as it is in reality distilled water, in the place of which it is also generally used by the apothecaries. I have been

using this water now for many years in my microscopical examinations, but I do not remember ever to have met with bacteria in it, though, if used unfiltered, minute fragments of vegetable tissue or other particles may be met with. Bacteria do not appear until putrefaction has set in.

A number of times I have had to listen to the remarks of medical friends that living germs might exist in the air, too minute to be seen by the aid of the microscope. To this I can only reply that the modern "germ-theory" has not been based upon "invisible" germs. Even Hallier and Pasteur, the most persevering germ-theorists, never attempted to substantiate their assertions by things they could not see. They only failed in proving that the germs or organisms which they did see represented the true cause of disease. The smallest organisms known at present are the monads, bacteria and vibrios, representing minute granules, either single or arranged in rows, as already mentioned. But minute as they are, they really require no high amplification to be perceived in the form of a dot; it is only for the study of their details that high magnifying glasses and correct illumination are required. And, as stated before, the modes in which they multiply have even been studied by the aid of the microscope. The modern microscope is fully able to show things much smaller than these organisms, regarded by the germ-theorists as the cause of the disease; and, unstable as the germ-theory may be, it is surely not based upon imaginary objects. Its failure only consists in its inapplicability to the phenomena associated with the pathological processes of the disease in question.

In the preceding pages I have endeavored to demonstrate that the cause of yellow fever cannot be assigned to the noxious influence of bacteria in the air, for the potent reason *that they are not met with in the blood and tissues* of yellow fever patients, either before or after death. Neither can it be assigned to the noxious effects of the breathing of air contaminated by the products of vegetable or animal decomposition, or to foul drinking water, for such a supposition would be, as I have endeavored to show, in opposition to all experience of modern science, teaching that infectious diseases never arise from these last mentioned

causes. There is, however, another "invisible" agent left, to the noxious influence and effects of which all the characteristic features of the disease may be referred. This agent represents a certain specific noxious poison, *being a product of the diseased organism itself*, and, in consequence, *of animal origin*. To show the probabilities of this supposition, and to explain the more than probable manner in which this poison is reproduced and increased in quantity, as well as transmitted from individual to individual, will be our next task.

When, in 1867, the yellow fever appeared again at New Orleans, and soon extended over the city in the form of an epidemic, I adopted the view then generally entertained by the larger portion of the medical profession and the people of this city, that the disease was non-contagious; and as the fungi-theory, originating in the statements of Klob and Hallier, was then prevailing among medical men, I also inclined to the view that yellow fever depended upon some kind of micrococcus floating in the air. And, in accordance with this idea, I examined the mucous membrane of the alimentary canal and respiratory passages in quite a number of cases at the Charity Hospital, but without discovering any micrococci or other minute organisms in those localities; in the same way I failed to discover anything abnormal in several specimens of blood from hæmorrhages of the nose occurring in some of my private patients, or from specimens taken from the dead body at the Charity Hospital. It was only in the black vomit, as already mentioned, that I met with fungi and their spores. But as it was obvious that the organisms contained in this fluid could not have any relationship with the primary cause of the disease, I did not hesitate to discard the fungi-theory, while retaining the view of the non-contagiousness of the disease. And, in view of the analogy certainly existing between some of the symptoms of miasmatic and yellow fever, I became inclined to regard the latter as a miasmatic disease in the most intense degree, depending upon a poison arising from a combination of decomposing vegetable and animal matters. A reason for this view was that the disease principally prevailed in cities in which large numbers of men and animals lived in close proximity along the borders of swampy rivers. This view, which is

still entertained by a number of physicians, I favored until the yellow fever epidemic of 1878 made its appearance.

Previous to this time my studies had been principally directed to the pathology and treatment of the disease; and, I must confess that, since I failed to demonstrate the primary cause of yellow fever upon the theory of a *contagium vivum*, I had paid but little attention to this subject afterward. But when I observed, during this last epidemic, the disease slowly extending, frequently from house to house—and, in many instances, after having once entered a house or a family, not leaving it until it had attacked every inmate who had not been affected by it before,—when I saw it traveling from one district of the city to another, new centers of infection arising in places to which the poison had evidently been carried; and when the epidemic slowly extended from our city to neighboring communities—especially along the railways and other routes of public travel,—even to villages and plantations in the country, my attention was again directed to its cause, and by subsequent and more systematic studies and observations I became convinced of its contagious nature.

It would be impracticable to enlarge upon this subject in this place by citing and discussing individually the numerous cases and instances which speak in favor of the contagiousness of this disease, I therefore only refer the reader to the innumerable reports, both of ancient and recent date, containing the history and statistics of yellow fever. I may, however, remark that any physician who will take the trouble and devote the time to an impartial and philosophical study of the subject in all its details, following the outlines which I have sketched in this treatise, will finally become convinced, like myself, of the contagious nature of this disease. My own observations and studies have even inclined me to regard the infectious poison of the latter as more subtle and virulent in its nature than that producing small-pox.

Yellow fever, then, is a disease, running a regular course, and accordingly depending, like small-pox, scarlet fever, measles, and other kindred diseases, upon a specific poison of animal origin, being a product of the diseased human organism itself. This assertion is proved by the characteristic feature which it presents

in common with all other specific diseases, consisting in the *immunity* from a second attack, imparted to the individual once affected; and this immunity from a second attack, which most strikingly distinguishes contagious diseases from all others, must be constantly kept in view in the study of the nature of the peculiar poison producing yellow fever.

The precise nature of the particular pathological changes which impart to the organism this immunity from the effects of the poison afterward, we are at present unable to determine. Perhaps the most popular view is that the poison alters the constitution of the blood in such a manner, and so permanently, as to be unable to make any further impression upon it afterward. It is equally difficult to say whether the changes are wrought upon the colored blood corpuscles in particular, or upon the plasmatic portion, the liquor sanguinis furnishing the nutriment to the organs and tissues of the body. Judging from the unusually rapid disturbance in the nutrition of the organism, manifested by the phenomenon of fatty infiltration and degeneration in a number of organs, and by the nervous disorders, the liquid portion of the blood would appear to be first affected; while, on the other hand, the facility with which the blood corpuscles seem to part with their hæmoglobin, as my observations have shown, indicates that these bodies must be likewise affected. For a number of years I have, in relation to the difference existing in the impression of the poison upon the blood in miasmatic and yellow fever, been in the habit of illustrating it in a somewhat homely manner by assuming that in the former the impression of the poison is only functional in character, and in consequence evanescent, while in the latter it is organic in its nature, and therefore permanent, and insuring the immunity from a second attack. But, though it appears to me possible that the immunity may depend upon certain permanent changes wrought by the poison upon the blood—difficult as it may be to explain—I cannot forbear to presume that similar changes may at the same time take place in the nervous tissues, rendering them in the same manner unimpressible for another attack.

Besides the immunity from a second attack, characterizing yellow fever as a disease produced by a specific poison, the clin-

ical symptoms, and, moreover, the anatomical changes observed after death, also speak for its peculiar and specific character. But as a specific disease cannot be produced in the human body by a general cause, it is obvious that the specific poison producing it must be derived from another human body affected by the same poison, and that this poison must, necessarily be a product of the organism of the affected individual. We may, therefore, presume that a *general* cause cannot produce a specific disease.

Yellow fever, when it first appears, never attacks a number of persons isolated from each other at one and the same time, but always, like every other contagious disease, starts from single centers, represented by the infected individuals; and the only manner in which it spreads is either directly from individual to neighboring individual, or indirectly, when the poison, emanating from the affected individual and adhering to clothes and other objects, is carried to a distant place and inhaled by another person. A number of centers may thus arise in different localities of a city, which, increasing in dimensions, may at last meet. In this manner, alone, the infectious poison is able to extend over a large city like New Orleans; and the surest proof of this assertion is offered by the slow march in which the disease walked over our city during the last epidemic. A purely infectious poison, on the contrary, being contained and distributed throughout the air of a city or locality, whether in the form of living organisms, or in the form of vegetable or animal effluvia arising from decomposing matter, will necessarily affect every person susceptible to infection nearly at the same time, and, in consequence, rapidly extend over a city or district. For this reason, the theory of a *contagium vivum*, or of the miasmatic or effluvial origin of yellow fever, is entirely inconsistent with the contagious character of the disease. But notwithstanding these existing circumstances, there are still a number of physicians who, while they recognize the contagiousness of yellow fever, entertain at the same time the incongruous idea that the cause of the disease is represented by minute living organisms, and for the sole reason, as they say, that this is the only theory upon which they can understand the increase of the poison, as nothing could spontaneously increase unless it were endowed with life.

I shall try to explain this subject by a suitable comparison. In every organized body, whether plant or animal, we meet with an albuminous substance which is regarded as the basis of life, and is generally known by the name of "protoplasm." This protoplasm, as we all know, forms the basis of all tissues performing a vital function, whether consisting of cells or fibers; and though it is not an organized body, it nevertheless represents living matter and is endowed with the power of appropriating other matter unlike itself—such as the nutritive matters derived from the food and contained in the blood,—and of imparting to it its own properties; or, in other words, of converting this matter into protoplasm. Now, it is upon this principle that all tissues and organs of the body not only rejuvenate themselves, but also increase in size. Thus, the protoplasm of a muscular fiber appropriates new matter and converts it into muscular fiber; the protoplasm of a ganglion-cell imparts its properties to the same new matter furnished by the plasma of the blood, and converts it into nervous tissue, etc. The new matter is in all cases the same, but it is the protoplasm of each particular tissue which represents the converting agency.

The specific poison, having entered the circulation, and being diffused throughout the blood, imparts, like the protoplasm, its own properties to this fluid, giving rise to all the pathological phenomena characterizing the disease. Still, it must be remembered, that the degree of intensity of the disease depends entirely upon the quantity of the poison taken up by the blood. If the quantity is sufficiently large to affect all the blood circulating through the body, the disturbance of nutrition would be so great as to interfere at once with all the vital functions, and the case would prove fatal very rapidly. We may, therefore, presume that, with some rare exceptions, in all cases of yellow fever the poison only affects a smaller or large portion of the blood, leaving to this fluid sufficient integrity to nourish the tissues and organs at least for some days, thus affording an opportunity to the organism to rid itself of the poison. This, however, can be accomplished in no other way than by means of the action of secreting, or, as we may say in this case, excreting cells, the same organs through which the organism rids itself of all other

foreign or wasted matter. Thus, the poison, already increased in quantity by that portion of the blood to which it imparted its own properties, is absorbed from this fluid by the cells of different glandular organs during its passage through the capillaries surrounding the individual minute glands. And, in passing through these cells, it again increases in quantity *by imparting its noxious properties to their natural secretion*, together with which it is finally eliminated from the organism. In yellow fever therefore, as in all other contagious diseases, it is in the secretions, especially those of the glandular cells of the skin and of the respiratory passages, that the noxious poison is contained, and with which it is removed from the system.

In some contagious diseases, the poison, before leaving the body, gives rise to certain pathological processes in the epidermic cells of the skin, manifested by divers exanthematous eruptions, which, in some instances, as in small-pox or the cow-pox, proceed to the formation of vesicles and pustules containing the poison in liquid form. Nevertheless, even in these affections, a portion of the poison leaves the body in a gaseous or vapor form, possessing the same virulent properties as the liquid contained in the vesicles or pustules. In yellow fever the poison emanates from the affected individual only in a vapor form, in which it may be communicated to another individual, or may adhere to surrounding objects, as clothes, bedding, furniture, etc. In the latter case, however, it may be supposed that while adhering to these objects it undergoes a certain condensation by drying, and in which state it is transported to distant places. But, as soon as acted upon by a certain degree of moisture and heat, it again assumes the vapor form in which it may be communicated to other persons.

In holding the view of the contagium of yellow fever or other contagious diseases being transmitted from one person to another through the vapor arising from the secretions of the skin and lungs, we need not presume that the whole vapor represented the noxious poison; on the contrary, we may concur with the opinion of Hiller, already cited, namely, that the ferments of putrefaction are represented by most minute protein-particles, which, being themselves in a state of putrefaction, are carried through

the air to the substratum to involve it in a similar decomposition. In the case of yellow fever, therefore, these proteïn-particles may be derived from the protoplasm of disintegrated epithelial cells of the glands of the skin to which the properties of the specific poison had already been imparted, and which, on their part, if entering the blood of another individual, are also capable of imparting these properties to the constituents of this fluid. This explanation corresponds with the theory that only solid bodies are capable of acting as ferments. Accordingly, the spread of the disease would be affected by the expansion of the vapor of the cutaneous secretion containing these particles into the air surrounding the patient, and inhaled by other individuals—and, furthermore, by the secretion adhering to the clothes and bedding of the patient, or to other neighboring objects; if the perspiration be profuse, as is generally the case, it will also be absorbed by the clothes and bedding.

In yellow fever as in small-pox the secretions of the skin are generally associated, as already remarked, with a peculiar odor. Whether this odor really represents the noxious principle, or whether, being somewhat ammoniacal in character, it is due to the decomposition of urea contained in the secretion, I will not pretend to determine. But, judging from its strength as it emanates from a single patient in a badly ventilated sick-room, we may form an idea of the facility with which the vapor, pregnant with the poisonous principle, may adhere to the clothes of visitors to be carried to distant places, or how easily it may communicate itself through the open windows to neighboring houses and people. But, in order to decide upon the probability of this mode of propagation of the noxious poison of contagious diseases, we should compare it with the manner in which odorous bodies impart their odor to the surrounding air, or to neighboring objects. Although nothing definite is known of the manner in which odors are diffused, or of the nature of the impression which they make upon the ultimate terminations of the olfactory nerves, it is nevertheless supposed (Cloquet),* “that many bodies possess the property of setting free from their substance exceedingly fine

* v. *Vintschgau*,—“*Physiologie des Geruchssinnes*”—*Handbuch der Physiologie*, herausgegeben von Dr. L. Hermann, Vol. III, part 2, p. 161, 1880. (Still under publication.)

particles, forming a kind of atmosphere around the odoriferous body and becoming diffused into the surrounding air; their density diminishes in proportion to their distance from the odoriferous body. Accordingly, the air represents the medium by which odorous substances are spread."

"*Cloquet, Bidder, Valentin, Longet, and Poinso†* have mentioned a number of conditions under which the odor is developed. There are, namely, odoriferous bodies the substance of which is incessantly, either partially or entirely, volatilized, while others only became odoriferous under certain circumstances. There are, for instance, plants which exhale only during daytime, or only during the night, or, even, only in the morning. Some plants emit odor when they are dried; a generally known example of this is found in freshly cut hay. Aromatic herbs possess in the dried condition only a feeble odor, but smell tolerably strong when moistened. Bituminous substances have no odor in the dry condition, but a distinct one when damp. The dampness appears, therefore, to favor the emanation of the odor, and still more, if the evaporation is supported by a moderate temperature; heat destroys the odor, and a low temperature arrests its emanation; these lower limits differ with different substances."

"*Liégeois* arrived at the conclusion that minute particles are constantly diffused from the odoriferous bodies, especially when they are in contact with water, in the atmosphere, and arriving at the Scheiderian membrane with the olfactory organ."

From these citations, then, we find that odors are produced by minute particles emanating from the odoriferous body to become diffused throughout the atmosphere, and their emanation is favored by dampness, especially at a moderate temperature. But there are also bodies of vegetable and animal origin which emit no odor, and yet manifest the same phenomena indicating the emanation of fine particles as from the odoriferous bodies. It is, therefore, not essential that a substance, in order to emit or set free minute particles, should be odoriferous. The tenacity with which odoriferous particles adhere to other bodies, especially such of a loose or spongy texture, is very great, so that in some

† l. c.—p. 262.

instances it is almost impossible to destroy the odor without the aid of water or chemical re-agents. The numerous examples of this kind, such as musk, castor, the secretion of the odoriferous glands of the pole-cat, assafoetida, etc., are sufficiently known as to require no further comment. It is only to the odor of the exhalations from the human skin, and to the tenacity with which the minute particles—emanating from the respective secretions from which the odor proceeds—must adhere to neighboring objects, that I wish to direct the attention. The most striking example of this kind is afforded by the dog tracing the trail of his master, or that of another animal in hunting. Many of the most remarkable instances are known and authenticated, in which dogs have traced the trail of their masters to great distances, even into neighboring countries, and which evidently show that a portion of the odorous particles contained in the exhalations of the master, must remain suspended in the air near the ground over which the trail passes, or adhere to the ground itself, notwithstanding the currents of air almost constantly present in the open streets or country. Nothing interrupts the trail but running water, which washes the particles away. In this instance, of course, the olfactory sense of the dog, perceiving these particles, is much more acute than that of man; this, however, does not invalidate the argument, as the particles must be there in order to leave an impression upon the dog's olfactory organ. Still more remarkable is the olfactory sense of insects, by whatever organ it may be represented, which attracts them to matters, which in many instances, even, possess no apparent odor, whilst they certainly must emit particles diffusing throughout the atmosphere to considerable distances to come into contact with the organs of special sense of these animals. It is a known fact, and one to which I can testify from experience, that in hot weather the odoriferous trail of a full-blooded negro may be perceived by the human olfactory sense for a considerable distance.

How strong and dense the exhalations of healthy persons even are, may easily be judged from the offensive odor of the air of small and closed sleeping rooms, if entered in the morning before they have been ventilated; and the tenacity with which these exhalations will adhere to linen clothes or other garments, may

be judged from the smell of those articles after having hung for some time in rooms overcrowded with people, as met with among the indigent classes of society.

Now, if we consider the amount of exhalation emanating from the skin and lungs of a yellow fever patient during the course of the disease, and moreover characterized by the peculiar odor already referred to, it must be obvious that the atmosphere of the room, even with proper ventilation, must become charged with the poisonous particles contained in the exhalation; and if there are several cases in the same house, the air within must eventually be contaminated with the poison; and attenuated as this poison may be, the particles representing it will surely adhere to surrounding objects, or escape through the open windows to be carried to neighboring houses. And, if we furthermore consider the complex nature of the intercourse kept up by the inhabitants of a large commercial city, like New Orleans, the facility with which a considerable portion of the poison, adhering to the garments of the nearer friends and visitors of a number of patients, will be carried to different places and houses must be easily understood by the philosophically thinking mind. For it cannot be supposed that every man, woman, or child, whose friends are lying sick in their homes with yellow fever, will break off their social and commercial intercourse with the rest of the people. In fact, this intercourse is so complex, and its threads so intricately interwoven, as to render the tracings utterly impossible, even to the sharpest detective, and at the very beginning of an epidemic. The many failures that have occurred in the attempt at tracing the first cases of yellow fever of an epidemic to their true origin are due to this complexity of social and commercial intercourse. But more especially to the fact, that it is almost always the *fatal* cases which first attract the attention of the authorities, while the *milder* ones, not seen by the physician, and perhaps not even recognized or suspected by the patient or his friends, themselves, and constituting the larger number, will get well without any medical aid. But, while the latter may not be noticed, they nevertheless reproduce and spread the noxious poison, as well as those terminating fatally.

In my paper on "The Nature of the Poison of Yellow Fever,"

published in the *New York Medical Journal*, May, 1879. I expressed the idea that the poison of yellow fever, similar to the virulent poison of putrid substances, increased in intensity with each individual through whom it passes. By a subsequent and more mature reflection, however, I have come to the conclusion that experience does not prove this supposition, as, otherwise, the intensity of the poison would soon reach a degree at which it would affect every person without regard to their degree of susceptibility—though it is not impossible that the virulence of the poison emanating from some individuals may be higher in degree than that emitted by others. But, even without this supposition, the quantity of poison emanating from any yellow fever patient, and possessing no greater virulence than that which originally entered his system, is sufficient to explain the spread of the disease.

As regards the transmission of the poison of yellow fever by ships from port to port which is, as mentioned before, a historical fact, it seems to me that even here the poison is generally carried by the garments and effects of the sailors or passengers, though I would not venture to deny the possibility of its being transmitted by the cargo, especially if the articles of which it consists had been exposed to the air of an infected locality. If cases of yellow fever have occurred during the journey, then there remains no doubt that the poison was carried by the clothing and baggage.

Regarding the decline and final disappearance of an epidemic of yellow fever, it is very generally believed that the noxious poison is destroyed by frost. The truth of this supposition, however, is only apparent, as it cannot be proved by any positive and reliable facts. On the contrary, it is a fact that, at New Orleans, the epidemics had almost always nearly disappeared when the first frost occurred, and that frequently new cases arise long after the appearance of this meteorological phenomenon. Thus, in the epidemic of 1867, I paid the first visit to my last case on the tenth day of December, a case which, though not proving fatal, was nevertheless a severe one, accompanied with jaundice and extensive hæmorrhages of the lungs, the patient being a foreigner who had arrived at New Orleans about two months before she was attacked by the disease. The same thing happened, as far as I

remember, at Memphis and other places situated more northerly than New Orleans, where the disease continued to prevail for some time after the appearance of the frost. Thus, the epidemic always reaches its maximum height a considerable time before frost appears, and there remains no doubt that its decline is chiefly owing to the decrease of the number of those persons susceptible to the action of the yellow fever poison, but also *to the lower temperature condensing the latter upon the objects to which it has adhered, and interfering with its evaporation, both from these objects and from the bodies of the affected individuals, and thus diminishing the chances of its being inhaled by other persons.* At the same time, the systems of those persons who thus far escaped the disease are reinvigorated with the approach of cooler weather in the month of October. These are, as I suppose, the chief causes to which the decline of the disease may be attributed, though the germ-theorists firmly believe that the frost kills the living germs, representing, as they suppose, the cause of yellow fever.

And to causes reverse to this may be attributed the re-appearance of the disease, especially in the year succeeding that of an epidemic, for it is very obvious *that during an extensive epidemic a considerable portion of the noxious poison must have remained adherent to clothing and to the walls of rooms, etc., as well as to mattresses and other bedding by which it had been absorbed with the perspiration of the patient, some of which, after having existed during the winter and spring—perhaps unexposed to the atmosphere and light, and in a state of condensation—may, with the returning heat of the summer, associated with moisture, be again evaporated and resume its activity.* There remains no doubt that in this manner the yellow fever epidemic, occurring in Memphis in 1879, arose from the clothing and effects of yellow fever patients from the epidemic of 1878, as soon as the shoemaker opened his box, in which they had been kept during the winter and spring. In the same manner arose those cases occurring in New Orleans in 1879, which, besides, might have increased in number to assume the form of an epidemic, if the board of health had not isolated them as far as circumstances would permit. All so-called sporadic cases arise in this or a

similar manner, and it is by no means improbable that the noxious poison of yellow fever, like that of small-pox, scarlet fever, measles, and other contagious diseases, may seldom become entirely extinct in places which it is accustomed to haunt, but constantly lurk about in dark corners deprived of fresh air, to resume its activity as soon as exposed to heat and moisture—and that, therefore, its fresh importation by ships is not always a strictly essential condition.

A further proof, however, that cold does not destroy the noxious poison of yellow fever, or of any other contagious disease, may be found in the known fact that low temperatures, on the contrary, preserve all organic substances. And to illustrate this fact we have only to point to the huge cadavers of the mammoth, buried for thousands of years in the ice-fields of Siberia, the flesh of which was so well preserved when discovered by the herdsmen of those icy regions, that they made use of it for feeding their dogs. But even if the cause of yellow fever were represented by living organisms, such as bacteria, it could neither be destroyed or killed by the cold of our winters, nor by a much lower temperature. This has been sufficiently proved by a number of experiments, but most forcibly by *Frisch*,* who exposed bacteria to a temperature as low as 87.5° C., below zero, a condition from which they revived with the thawing of the liquid.

Much has been said and written about certain meteorological conditions required for the activity and propagation of the yellow fever poison, such as a certain amount of moisture in the air together with a continued tropical heat, a deficiency of ozone, etc. Though nothing positive concerning these conditions has thus far been ascertained, I would not deny the probability of such particular conditions; for the fact that yellow fever has assumed the dimensions of extensive epidemics only at shorter or longer periods, while, on the other hand, it has frequently existed in the form of so-called minor epidemics, or been represented only by a few sporadic cases, shows that the same atmospheric conditions are not always present. Neither does the noxious poison,

* *Virchow u. Hirsch*, Jahresbericht fuer das Jahr, 1877, Vol. I., p. 285.

after its absorption into the blood, manifest its activity in the same degree in different persons, a phenomenon to be explained by the different degree of susceptibility which these persons possess to the effects of the poison, depending probably upon the particular state of their constitutions at the time when it is brought in contact with the latter. Thus we frequently meet with persons who have been exposed in many different ways to the action of the poison during several epidemics without ever being affected, but who, after the lapse of many years, are finally attacked by the disease. This was the case with myself; for while I escaped taking the disease in former epidemics, I became infected in 1878, while I was engaged in examining the blood of living patients at the Charity Hospital, and when, in order to obtain the specimen of blood, I was obliged to remain for some time in very close contact with the patient, thus inhaling his exhalations. In such cases the disease mostly appears in the milder form. Nothing is known of the nature of this singular immunity, whether it is owing to a peculiar constitution of the blood or of the nervous system at the time of exposure. Nevertheless, it has at times been taken hold of by the "non-contagionists," and forwarded as a proof of the non-contagiousness of yellow fever. Such an argument, however, must fall to the ground in considering that the same phenomenon is observed equally as well in scarlet fever, measles, and small-pox, etc., the contagiousness of which is universally acknowledged. Thus, a number of years ago, I attended a little girl about seven years old, affected with confluent small-pox; it was the most severe case of which I ever had charge, for the eruption extended from the face over the whole scalp, almost forming a mask, and was associated with considerable suppuration and a loss of the entire hair. In this case, remarkable as it may appear, the mother of the patient had during the whole course slept with it in the same bed, and the grandmother in the same room, without either of them contracting the disease, while the father by only once approaching the bed, at the time when the pustules were drying, rapidly caught the disease which fully developed upon him. As far as our knowledge extends at present, we are unable to offer any explanation of these contradictory facts, and it appears to me useless to indulge in empty speculations.

Although the customary assertion that native-born or long acclimated persons enjoy immunity from an attack of yellow fever has now been sufficiently proved as unfounded, it cannot be denied that, in most instances, acclimatization greatly ameliorates the severity of the attack. This is most strikingly observed in unacclimated persons, especially in those who have lived in northern latitudes, beyond the yellow fever zone, and who are generally the first victims to the disease. And it has become a general rule, corroborated by all writers, that the nearer a person has been born and lived to the North Pole, the more liable he is to be attacked by the disease, and the smaller his chances will be for recovery. The fallibility of this assertion, however, has also been shown by our experience during the epidemic of 1878, when the French, Italian, and Spanish population of New Orleans suffered equally as much, *if not more*, from the disease than that of Teutonic or Anglo-Saxon origin; even the Negroes did not remain exempt. Nevertheless, as regards non-acclimated or acclimated persons, it is not improbable that the air of the yellow fever zone may in some essential points differ from that of northern climes, enabling it to exert a certain unknown influence upon the constitution of the blood of those persons who breathe it, adapting it, so to speak, to the yellow fever poison. And to this circumstance it is mainly due that the rate of mortality during the epidemic of 1878 had been proportionately much larger in Memphis, Vicksburg, Holly Springs, and numerous other places, than in New Orleans, or even in other parts of Southern Louisiana. Besides this, the proportion of persons who enjoy an immunity of the disease, by having been previously affected by it, to the whole population, is greater at New Orleans than at the places above mentioned.

TREATMENT.

Although from the facts relating to the clinical phenomena, pathology, and probable nature of the specific poison of yellow fever, which I have presented to the reader in the preceding sections of this treatise, a rational course of treatment may be easily deduced, some additional remarks will be demanded to direct the attention of the practicing physician to certain apparently minor, but really important points.

The most simple method of arresting a disease, produced in a previously healthy organism by the noxious effects of a specific organic poison, would, of course, consist in the neutralization of the latter, inducing a cessation of the pathological processes—if not advanced too far—and a gradual return of the organism to its normal condition. Unfortunately, however, medical science has as yet not discovered the antidote of the yellow fever poison, and we must, therefore, look for another method of treatment, by which we may counteract the effects of the poison, and, moreover, assist in its elimination from the system with as little damage to the patient as possible—a treatment, which, if based upon rational principles, should correspond to the natural efforts which the organism is observed to make in the elimination of other infectious poisons. Numerous examples of this kind are constantly witnessed in the so-called critical sweats, diarrhoea and other discharges, or in the eruptions of some other contagious diseases; for, it would be difficult to prove that these processes, though pathological in character, do not represent the effort and the means by which the unaided organism chiefly rids itself of the foreign intruder, the morbid poison. In taking a simple case of intermittent fever as an example, we meet with many of the phenomena as are witnessed in the first stage of yellow fever, but which, in this instance, will disappear with the termination of the sweating stage. The most rational inference to be drawn from this fact, therefore, is that the poison, or, at least, *the product resulting from its combination with one or the other constituents of the organism*, is eliminated by a secretory, or rather excretory, process of the glands of the skin, or, perhaps, also by the biliary or urinary secretions. The same phenomenon may be observed in small-pox, when the fever abates with the formation of the vesicles, and ceases as soon as the whole eruption is out. From this assertion, however, it must not be inferred that an excessive secretion of the skin, or of the liver and kidneys, will at once eliminate the poison from the system, but it must be remembered, that every gland can, in its normal condition, only perform a certain fixed amount of labor, proportionate to the quantity of its parenchyma, and that any increase obtained by means of artificial or other abnormal stimulation must be followed by a

decrease of the normal amount. Besides, some other points and conditions associated with the febrile process, and incompatible with an excessive secretion, must also be taken into consideration. The secretory functions, therefore, should only be moderately promoted if possible, but never be stimulated to excess. The next object in view is to reduce the elevated temperature to near the normal standard for the special purpose of limiting the disintegration of the albuminous substances, which, as is believed by a number of pathologists, is promoted by an abnormally elevated temperature of the body. The third, and perhaps most important point to be observed, however, is to prevent or to allay every excitement of the nervous system.

In passing a review of the clinical phenomena and the pathological anatomy, we find that in all fatal cases the nervous system takes a most prominent part, and in discussing the general pathology of the disease I have endeavored to clearly demonstrate this fact. And it may safely be pronounced that if the integrity of the nervous organs were not disturbed, the organism, if left to itself, would easily get rid of the poison, and the fatality of the disease be removed. This assertion is fully proved by the thousands of cases which recover without any medical aid. The most dangerous of these nervous disturbances is the hyperæmia of the brain, which, as I have shown, is almost always observed in fatal cases after death. For this reason the physician should never neglect to study the condition of the brain of his patient, from the various nervous phenomena manifesting themselves during the course of the disease, particularly during the febrile stage, and by the proper therapeutic means and applications endeavor to keep the congestion of this organ confined within safe limits. The first condition, essential to the safety of the patient must, therefore, be *the perfect rest of the mind, as well as of the body.*

To secure the necessary rest of the mind for the patient is in many cases quite a difficult task for the attending physician on account of the pernicious custom, especially prevailing among the working classes, of the female friends and neighbors entering the sick-room, and going to and fro for the pretended purpose of inquiring after the condition of the patient, but in reality from idle

curiosity. I doubt not that many a case of yellow fever has terminated fatally for the want of the proper rest of the mind, and the proper nursing at the *proper* time. It usually happens that when there are too many friends the patient is neglected, and will have to pay the cost with his life. For this reason, the physician, after having taken the case in charge, should at once select one or two of the most intelligent and trusty friends, who voluntarily offer, to stay alternately with the patient, day and night, at least until the crisis is over. Much has been said about professional yellow fever nurses. I have never had a great deal of faith in these persons, as they are strangers to the patient, and frequently too meddlesome, imagining that they know better than the physician, and not carrying out his orders very strictly—not infrequently even supplanting them by their own notions. The most reliable nurses are always the nearer relations and friends of the patient, whose sentiments and interests are interwoven with his own. The impression made upon the patient, by seeing strange faces around him, can neither be favorable. In the epidemic of 1867, particularly, I observed the marked importance of conscientious and careful nursing in a number of severe cases, who, attentively nursed by their nearer friends and relations, recovered, while others, surrounded and nursed by strangers, succumbed.

As soon as the first symptoms of the disease appear, therefore, the patient should be confined to bed and made as comfortable as possible, both in body and mind. The physician should contribute to keep up his moral courage by representing to him his case as light as possible, and by explaining the importance of the rest of his mind. Intelligent patients I have usually advised to keep from using their mind at all, but merely vegetate, so to say, in trying to sleep, and to leave all cares to their friends. Regarding the sickroom, it is of the greatest importance that it should be well ventilated, as otherwise, not only the patient, but also those persons who nurse him, will be obliged to breathe his cutaneous exhalations containing the infectious poison. At the same time, however, every direct current of air upon the patient should be scrupulously avoided; he should be lightly covered, but his surface not wantonly exposed.

In discussing the general pathology of the disease, I referred to the probable temporary increase of the biliary functions in the commencement of the disease, and, for this reason, the customary initiation of the treatment by the administration of a mild cathartic, for the purpose of unloading the portal circulation, must be recommended. There are a number of medicines which will answer this purpose. The most popular one in this city, both with physicians and the people, is castor-oil, and, if not rejected by the stomach, answers as well as any other on account of its quick action. A number of physicians administer a dose of calomel, combined with one or the other cathartic, such as jalap, etc. As the calomel produces to a certain extent a bilious stool, this remedy rather appears favorable, especially as we know from the autopsies that in almost all fatal cases the gall-bladder contained a dark brown, tar-like bile. The citrate of magnesia may also be used, though it is frequently rejected by the stomach. Subsequently, in the course of the disease, however, it is better to use an enema instead of the cathartic medicine.

The almost universal custom of giving a hot stimulating foot-bath for the purpose of inviting the blood from the brain to the feet, also, may be of service in the beginning of the disease, but with the necessary caution to be administered after the action of the cathartic, allowing the patient to remain undisturbed afterward. The foot-bath, however, will only prove of advantage in the beginning of the disease.

In order to promote the perspiration, it is customary—at New Orleans—to administer an infusion of orange leaves. These leaves, if possessing diaphoretic properties at all, possess them only in an inferior degree. They are simply used in Louisiana, because they are easily obtained. In the Northern States they would be replaced by “boneset,” (*Eupatorium perfoliatum*), and in Germany by the flowers of “chamomile.” And as such infusion, in order to produce a diaphoretic effect, should be administered while hot, but which, in most cases, is neglected, the benefit derived from the orange leaves is only imaginary; even if the cold infusion is taken with the view of allaying the irritation of the stomach. Besides, the administration of hot infusions at a

time when the patient suffers already from the excess of heat, both from the weather and the disease, is hardly practicable, as it really contributes nothing to the comfort of the patient. Perhaps one of the most efficient and harmless remedies is the "neutral mixture" with the addition of a little ($\frac{1}{16}$ part) sweet spirits of niter, given in such doses as to keep the skin only in an evenly moist condition, which, however, may be discontinued while the skin remains moist, but resumed as soon as the perspiration ceases, and the skin becomes again dry. This mixture is tolerably well borne by the stomach, whilst the spirit of niter, besides, may slightly stimulate the kidneys. But as with the rise of the fever the thirst of the patient rapidly increases, he finally also refuses this remedy, and demands ice-water, which, if given in proper doses, appears to suit the occasion as well as any article the apothecary could furnish. Unfortunately, however, the thirst of the patient is so great that he loses his self-control, and will fill his stomach with the cold liquid if not interfered with by his nurse; then, small fragments of ice suffered to slowly dissolve in the mouth may answer the purpose.

[TO BE CONTINUED.]

ARTICLE VII.

MASSAGE. By JAMES I. TUCKER, A.M., M.D. Chicago.

Inasmuch as massage is attracting considerable attention now-a-days, and is thought by many to be a notion of modern times, it occurred to me that the following medico-historical data * might be no less interesting to your other readers than they have been to me.

In India the massage method was in vogue at least two thousand years ago, for a Greek historian, who was in that country 300 years B. C., related that at that time there existed among the Brahmins an order of physicians whose treatment consisted in regulating diet, enjoining abstemiousness and outward manipulations. At the present time, in the valley of the Ganges, there is an order of Brahmins, whose method of treating disease is a hygi-

* See Gartenlaube Heft 14, 1881, p. 445.

enic rubbing and shampooing after the bath. The patient lies outstretched upon a lounge, and the Brahmin kneads his limbs as if they were dough. Then he strokes the body lightly with the palms of his hands, perfumes and anoints him, and concludes by bending and stretching the neck and joints of fingers and toes.

The same method is in vogue in the Orient, especially among the Turks. The beneficial effects of massage were likewise known to the ancient Greeks and Romans, for after their bath they had their slaves rub, knead and anoint them, and Hippocrates considered it important that the physician, among other things necessary to the mastery of his art, should understand massage. Asclepiades, who lived one hundred years before Christ, was likewise an advocate of this method of healing.

It was a Swede named Pehr Hewrik Ling, who, while founding a system of gymnastics upon purely anatomico-physiological principles, introduced passive motion. This occurred in the second decade of this century, and the students of Ling brought the massage method to great perfection. For a long time all kinds of passive motion, like stroking, patting, pounding, kneading, rubbing, pinching, stretching, etc., have been applied with great success.

Of late a Hollander named Metzger, who has achieved brilliant success by the help of massage, attracts to himself both the learned and the laity, some to place themselves under treatment, and some to learn his method.

His method consists principally of four kinds of manipulation, to wit :

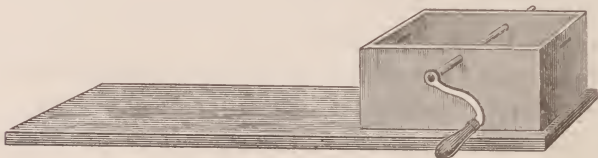
1. *Effleurage* : slow and gentle stroking with the palms of the hands.
2. *Massage à friction* : forcible stroking and rotary rubbing movement in alternation, either with one hand or with both hands.
3. *Pétrissage* : kneading.
4. *Tapatement* : pounding, beating and slapping with the flat hand or its edge, and with the closed fist.

So important an adjunct to the medical art as massage, that rests upon a scientific basis, and endorsed by so many German, French, English and American physicians, including Esmarsh, Charcot and Mitchell, should not be overlooked by any of the members of our profession.

ARTICLE VIII.

A PRACTICAL BANDAGE ROLLER. By NELSON H. CHURCH, M.D., Chicago.

In the *Medical Record* of July 16 inst., appeared an item entitled "An Easy Way to Roll Bandages." I write to approve of the method, having used the same for ten years. Last winter I thought of improving upon the plan. I made an apparatus, which is not much of an improvement, though it is decidedly more convenient. It is described as follows, viz.: A wooden



box, measuring on the inside $2\frac{1}{4} \times 3$ inches, and two inches in depth (or deeper if desired). Two holes were made through the bottom of the box, one in the middle and one near the end. These holes were large enough to admit a wire, as large as a No. 5 catheter. Now take a piece of board one inch thick, four or six inches wide, twelve or eighteen inches long; make two holes in the end and corner of the board to correspond with the holes in the bottom of the box; through these holes put the wire which has previously been bent at right angles—one angle to be half an inch long, the other to be one and a half inch long and to have a thread cut on it (which can be done by a gunsmith); have the short angle go part way through the board, the other to go through and fasten with a screw nut, or, what would perhaps be less trouble, make a groove in the bottom of the board deep enough to accommodate the long end of the wire when bent to the proper angle. This manner of fastening is preferable to screws, because it will not work loose nor split the box. It is not necessary that the box should be fastened to a board; it is only suggested to save mutilating a desk or table. One can sit on this piece of board

while rolling the bandages. When one has a table that can be used for the purpose, holes can be bored in one end in the corner, the box fastened and left *in situ* for immediate use. I prefer to have the roller fastened to a table because it is firmer than the board, and one can handle the bandage more readily. The size of the box will admit of rolling bandages wide enough and long enough for any ordinary purposes. The crank is better made of wire the size of a No. 5 catheter and put through the middle of the box, in either diameter, about half an inch from the top; this will admit of rolling a long bandage without touching the bottom.

When rolling a bandage make pressure on the crank, keeping it well in place, and hold the bandage quite firmly over the edge of the box. The end of the bandage should be slightly wetted before commencing to roll, as that makes it adhere to the wire just enough to prevent slipping. After the bandage is wound, pull out the crank, or if the raveling should hold the bandage fast to the wire reverse the crank a few times and withdraw it.

With the smallest amount of practice one can roll a bandage very quickly, very evenly, and very firmly.

PRIZE ESSAY.—The Committee of Selection appointed by the chairman of the Section on Practical Medicine, Materia Medica and Physiology, at the recent meeting of the American Medical Association, have selected, and hereby announce, as the subject for the prize to be awarded in 1883, the following question:

What are the special modes of action, or therapeutic effects upon the human system, of water, quinia, and salicylic acid, when used as anti-pyretics in the treatment of disease? The essays must be founded on original experimental and clinical observations, and must be presented to the chairman of the committee of award on or before the first day of January, 1883.

N. S. DAVIS,	} Com. of Selection.
H. D. HOLTON,	
W. B. ULRICH,	

Clinical Reports.

ARTICLE IX.

SURGICAL CLINIC OF PROF. ANDREWS. AMPUTATION OF THE KNEE JOINT.

Amputation at the knee joint is an operation which has been unaccountably neglected in America and, in fact, the world over.

Dr. Brinton, in 1868, published (*Am. Jour. Med. Sciences*) statistics of only four hundred cases of this amputation as a result of inquiries into the operations of the whole world. Ten years later I reported the search among the published statistics of both continents showing five hundred and fifty published cases. Since that time, although the operation has made progress in public favor, it has remained somewhat rare.

The following cases recently occurred in Mercy Hospital :

11,134. Female, aged 48. Entered hospital with a severe form of tertiary syphilis. The right leg was a mass of ulcers. The left one was similarly but less severely affected, being spotted with suppurating tertiary sores. Similar ulcers existed on other parts of the body to a limited extent.

The treatment consisted of iodide of potassium and local anti-septic dressings for twenty days, when it was found impossible to save the limb and it was amputated through the knee joint, after the method of Dr. Stephen Smith.

The following cut shows the line of external incision in the bilateral flap amputation recommended by him in his *Operative Surgery* :

Beginning about one inch below the tubercle of the tibia and, cutting to the bone the knife is carried downward and somewhat



forward around the curve of the limb when it is turned somewhat backward and the incision terminates in an upward direction just beneath the line of articulation in the popliteal space. The flaps are similar except that the inner one is made somewhat more redundant to allow for the greater prominence of the inner condyle.

These flaps are made to include everything down to the bones, and when they have been carefully separated and raised, the ligaments of the joint are divided and the leg separated, only a scalpel being required to complete the cutting.

The older plan of removing the articular cartilages, or of sawing off the condyles is to be condemned in emphatic terms. In practice it is found that the much dreaded inflammations of the remaining joint surface is not met with if antiseptic treatment is effectively carried out. The patella is also left *in situ*, and never transposed to the fossa below as formerly recommended.

This patient left the hospital twenty-five days after the operation with the stump almost entirely healed by first intention.

Case 11,339. Male, aged 40. This man suffered from long standing necrosis in the upper part of the tibia of his right leg. In 1864, while a soldier, after great fatigue and exposure from sleeping upon the ground, he began to suffer from an inflamed knee and great lameness. This continued about one year, when an abscess formed upon the external aspect of the limb just below the joint, and there had been a running sore ever since. The head of the tibia was much enlarged and there was such extensive necrosis in its upper third that I agreed with the patient in preferring an amputation to an excision of the joint.

The amputation was performed as in the last case, leaving the patella which showed no implication in the disease, and the joint surfaces with all the cartilages of the femur. The external

condyle of the femur showed plainly by its inflamed condition, that there had been synovitis; nevertheless, no part of it was removed, and not much trouble arose from this fact in the healing. Much time was taken to ligate all the small arteries about the joint, because of the excessive anastomoses and vascularity of the parts from the previous inflammation.

The operation was performed under spray, and the wound treated with Listerian dressings carried high up the thigh.

A very speedy recovery was obtained under anodynes, iron and cinchonidia, with a temperature at maximum during recovery of $101\frac{1}{2}^{\circ}$ Fahrenheit.

Five weeks after the operation a slight hæmorrhage occurred from the wound, and at the same period severe pains and swelling over the outer condyle; a small abscess had developed at this point and was opened down to the bone. No caries appeared to be present. This opening was kept widely open, and there was no recurrence of the pain. Two weeks and a half later, or seven and one-half from the time of operation, the patient left cured.

Case 11,256. Male, aged 30. Attempting to climb upon a moving freight train, this man fell under the wheels and had his left leg crushed off at the upper third.

An hour after the accident his pulse was 120. An amputation was decided upon at the knee joint, after some hesitation in regard to the vitality of the tissues destined to compose the inner and external flaps. The skin and superficial fascia were found loosened at one part as high as the lower third of the thigh from their attachment to the parts within. Ultimately abscesses formed as high as the upper third, which caused some trouble but no serious delay in the healing.

Three hours after the accident the knee-joint amputation, as above described, was performed by Dr. E. Wyllys Andrews. The flaps were scanty and the external one bruised and torn.

Forty-eight hours later a small triangular segment of this flap was found to have mortified, and the wound gaped open at this point leaving the condyles of the femur exposed.

The temperature was for several days 103° at evening, and pulse 130.

The abscesses in the parts above were thoroughly carbolized

by means of injections through a soft rubber catheter beneath the flaps and through the counter openings.

At the present time—seven weeks after the operation—the exposed parts are covered with rapidly-growing granulations, and the contraction of the cicatrix bids fair to draw in the surrounding parts so as to furnish a more or less complete tegumentary covering to the stump.

The advantages of this operation are such as to commend it most strongly where other indications are suitable. First of all, it is far less dangerous than amputation in the lower third of the thigh. A greater length of limb is at the same time secured and one which gives the best support to an artificial limb. In this respect it is superior even to the stump after a leg amputation.

It is the only stump upon which the entire weight of the body can be carried without pain or ulceration. The scar left is surprisingly small, and is in the posterior aspect where it does not interfere with the use of any artificial appliance. There is no cicatrix upon the end of the stump at all. No bone being divided in the operation, which requires for its execution only a strong scalpel, the danger of osteo-myelitis is not to be counted among the risks attending it. The supposed dangers from leaving in the wound a large synovial surface are from *a priori* conclusions and are wholly mythical.

NOCTURNAL INCONTINENCE OF CHILDREN.—Prof. S. D. Gross, of Philadelphia, advises the following formula :

Strychniæ	gr. j.
Pulv. cantharides.....	grs. ij.
Morph. sulph.....	grs. iss.
Ferri. pulv	scr. j.

Mix. Make 40 or 50 pills or powders, *pro're nata*. Sig.—One three times a day to a child ten years old.

This prescription will speedily relieve the irritability of the bladder, especially if conjoined with such means as a cold shower bath daily, the avoidance of irritant food and late suppers, the patient lying on the side or belly, and taking care to drink nothing for the few hours preceding sleep, and to empty the bladder on going to bed.—*Mich. Med. News*.

Hospital Reports.

ARTICLE X.

COOK COUNTY HOSPITAL.

CASE OF HYDROPHOBIA. Service of DR. NORMAN BRIDGE.
Reported for THE CHICAGO MEDICAL JOURNAL AND EXAMINER, by H. D. VALIN, M.D.

Peter D., a Norwegian laborer, on admission to the hospital Aug. 10, gave the following history: Father died after a surgical operation for necrosis of the tibia; mother and two sisters are living and healthy. Three children died during infancy. Patient was sick when three years old, but does not know the nature of his sickness. Had jaundice seven years ago. He has been stabbed about the face, head, back and extremities in various brawls. Has been a hard drinker, using beer to excess, and occasionally drinking whisky. Last March he was bitten on the left hand by a small dog, which was killed immediately, as a matter of course. The wound healed within three days, without a physician's care.

Monday, Aug. 8, patient worked as usual. During the afternoon he noticed that his head felt heavy, and soon began to ache, in the posterior part especially. He ate his supper, and rested well that night.

The 9th, in the morning, he ate some breakfast and drank one cup of coffee. He tried to drink another, but could not, and felt sick, with a severe headache. He lay down, with the intention to go to work at noon. At 9 A. M. a friend brought him some beer, which he attempted to drink, but could not. He then tried water, which at once made his throat feel as though he were chok-

ing. After that he could not look at water without the same feeling. Aug. 10, at night, he could not sleep well, and the 11th, in the morning, he felt worse. He said that he was smothering, and his head ached. Went out to see Dr. Fenger, and the wind made him almost wild; he had to stop often to get breath. He could not drink water or look at it without having spasms of the muscles of the throat come on. States that the place where he was bitten has caused him no pain at any time.

On admission, the 11th, he complained of pain in the head, at the base of the posterior part. A slight draught causes tremor of his whole body and a smothering feeling. Upon attempting to drink water he makes convulsive movements, and states that he feels as though he was smothering.

He had altogether, in the afternoon of the 11th, 80 grs. of chloral, in 12 and 15 grs. doses. At 7 P. M. he took 9 grs. of bisulphate of quinine. The retinae were normal, as per ophthalmoscopic examination of Dr. Jacobson. At 10:30 P. M. he took fifteen grs. of chloral, and soon after passed his water.

At 10:45, took a drink of water by himself, a marked improvement, seemingly due to chloral. 11:30 chloral gr. xv.

Aug 11. 12:45 A. M., pulse 118. Patient cannot sleep, and takes his medicine with difficulty. All his symptoms were more marked.

2 A.M. Pulse 120.

3 A.M. A dose of chloral.

3:30. Patient was raving. Took another dose of chloral. No sleep during that night.

9:30 A.M. Chloral, gr. xv hourly. Bowels moved.

10. Pulse 128, temperature 101.8.

12 M. Half a grain of morphine hypodermically.

3:40 P.M.

℞. Potassii bromid.....3 j

Chloral hydrat..... gr. xv M.

one dose.

4:20. Attendant had to put on the straight jacket to keep the patient quiet.

4:25. Chloral, gr. x hypodermically.

4:53. $\frac{1}{2}$ grs. of curare was given hypodermically. The

spasms came on and passed away every few minutes. Patient tried to drink water through a rubber tube four feet long, and required the goblet to be held out of sight. He did not succeed in drinking. He assumed a quarrelsome countenance, and said that he would not drink, because that would kill him, he thought. He made great efforts at expectorating, and his cough had that peculiar roughness which is also noticed in delirium tremens, and not quite unlike that of croup, though it did not resemble much the barking of a dog. Although the patient's hands and feet were tied, and he spat on his guardians, he made no effort at biting, which would naturally suggest itself in such circumstances. Although raving a part of the time, he would also occasionally speak rationally.

5:45 P.M. $\frac{1}{6}$ gr. curare hypodermically.

5:50. Chloral hydrate, gr. v., in the same manner. The patient seemed to quiet for a little while under the influence of curare, and he looked as if the disease had as yet but slightly changed his features.

6:25. Curare, gr. $\frac{1}{6}$ hypodermically. Patient soon quieted down, but still muttered.

6:35 P.M. Pulse 180. Temperature $109\frac{1}{4}$ in the rectum.

7 P.M. He breaks off the straight jacket. Pulseless at the wrist.

7:05. Aqua ammonia, 5ss hypodermically, and also by inhalation.

7:08, temperature 109.5 , pulseless.

7:10. Dead.

This case has this remarkable character, that the incubation of the disease lasted over four months, a time at which some authors believed that all dangers are passed. The duration of the disease was the average. The treatment with chloral and curare seemed beneficial.

It has been alleged that cases of death by curare have taken place, but in this we see that ten minutes before death the patient had strength enough to break off the jacket, and did not die paralyzed.

Foreign Correspondence.

ARTICLE XI.—THE LONDON MEDICAL MAN.

MESSRS. EDITORS :—In these pages I will endeavor to describe to you, so far as lieth within me, the London Medical Man in all his stages, from all his aspects. I will touch especially on his education—preliminary and medical—his earlier years of practice, the divisions of practice, his finances, his way of doing business, his manner of living, discouragements, enjoyments and social position. I speak from five months residence in London, in daily contact with the London medical man and his work in the various hospitals, schools of medicine, and private practice, and his social world. Also, from having interviewed men in the various divisions of the practice, and the various stages of life ; also those who look on and see from an impartial and unbiassed standpoint.

To have a good basis, we will allow that he is born at full term, the legal child of virtuous parents, a vertex of the 1st ; that he cries lustily ; that the cord is tied and cut *secundum artem* ; that he is carefully washed, and, as soon as the doctor leaves, given butter and sugar, and, possibly, a “dhrop of spirits.” All this he survives, as does he also, cake, buns, beer, pickles, and “just what we ourselves have ;” conquering, also, the dessicating cord, the thrush, the ruthless vaccination, the perils of the first summer, teething, measles, whooping cough, scarlet fever, chicken-pox, mumps, knives and breeches. He lives on despite the school-board, the school-marm or master, the vitiated atmosphere of the school room. Rolls, rules, after hours and lessons affect him not. He is born to be a doctor, and not to die in infancy or childhood as do thirty per cent. of his fellows. He

follows this schooling till the age of sixteen years. He becomes eligible to register and make his matriculation examination and enters the study of medicine. He either does this, or, as a minority do, goes on taking the degree of B.A., or B.Sc., at Oxford, Cambridge, London, or some other university.

Just here may be given the two great divisions and the four subdivisions of those who in the United States would come under the title of doctor. First are the physician and surgeon. Then the general practitioner of medicine, the consulting physician, the general practitioner of surgery, and the consulting surgeon. These four divisions are quite definite.

The general practitioner of medicine generally does not have a literary degree, but passes his matriculation examination at the Royal College of Surgeons, Royal College of Physicians, Apothecaries Hall, one of the Universities, or at the Hospital Medical School at which he enters, or one of the numerous other places. As an example of their nature I will quote the requirements of the matriculation of the Royal College of Surgeons: a certificate of having been registered by the General Medical Council; writing from dictation; English grammar; writing a short English composition; arithmetic, geography, English history, mathematics; algebra, through simple equations; Euclid, books I and II; translation from *Cæsar de Bello Gallico*. These are compulsory, besides which are a number of optional subjects. His studies are only recognized from the time of his registration. He may have studied any length of time before his name is placed on that public register but it is no good as to time. He then either goes to some recognized provincial hospital for eighteen months, or goes as the pupil of some legally qualified and registered practitioner for the same length of time. Said practitioner must be connected with a hospital, dispensary, or hold a poor-law appointment, so as to be enabled to give his pupil instruction. Not more than eighteen months of such study as the above is recognized by any of the licensing bodies. Such study is becoming more infrequent and unpopular, the student generally going immediately into his hospital medical school. It is strongly recommended that the pupil pass his preliminary examination before endeavoring to do anything at medi-

cine, so as not to have too many irons in the fire. At least two and one half years of the four years required must be spent at a hospital medical school, including three winter and two summer sessions, the former six months the latter three months each. Thus the English *annus medicus* equals nine months. A hospital, to be recognized by the licensing bodies, must have 150 beds. All of the first and a greater part of the second year he spends with the bones, the anatomy, in the physiological laboratory and dissecting room, practical pharmacy, lectures on materia medica and botany, peeping occasionally into the wards and out-patients' rooms, and of course being a regular attendant on the "capital operations," and prepares for "the first college exam." This passed he enters regularly into the study of medicine. He clerks, or, as the English say "clarks," with the *a* as broad as you can make it. This he may do in either the wards, out-patients, or special departments. It consists of examining the patients, taking notes or histories, and generally assisting the physician in charge and endeavoring to answer all the questions put to him by said physician. This he must do for six months. He hears lectures higher in the scale, including medicine, surgery, midwifery and gynæcology, instruction and proficiency in vaccination, pathological anatomy, forensic medicine, and does post-mortem work. Having completed four years study under the above regulations, he is permitted to come up for his pass examination. A large per cent. fail the first time and are remanded for three or six months. There are nineteen licensing bodies in Great Britain, and he has his choice. None of the applicants for license are allowed to be examined by their own teachers. A majority do not get into practice for five years. The general practitioner after receiving his license generally settles down to business in some modest house in a modest street, and takes unto himself, probably a modest wife. He usually takes the license of the Royal College of Physicians, or the license of the Society of Apothecaries, with probably a surgical degree. During the first two or three years after this he takes the M.D. degree, from Oxford, Cambridge, London, Durham or some other of the universities.

As to the consulting physician the differential diagnosis is this:

He is a little more ambitious in the start and his ambition and patience grow and increase. He also usually commences with more money—the average student probably commencing with from \$2,000 to \$3,000. He takes the B.Sc. from the London University, or is a graduate in arts from some of the universities in the majority of cases. Usually spends five years before he takes his M.B., or M.R.C.P. Practice is a thing he does not bother about. He hangs around his hospital or other hospitals. If sufficiently influential, or expert enough in wire pulling, he gets a house physiciancy, may be a house surgeoncy, or he may go through both these positions, also that of midwifery, assistant or surgical registrar, or chloroformist. He looks out for assistancies in the special hospitals of various kinds. He endeavors to put in about three years in work of this kind, making in all probably eight years before his brass plate goes on his door. During these years his love for the study which first engaged his studious attention is paramount, and he takes on letters whenever possible. For instance, a young man who recently, in London, attained to a position of honor, writes after his name, A.M. M.B. (Cantab.), M.R.C.S., Eng., and lives. The love of the London medical man for the alphabet is wonderful.

The general practitioner of surgery and the consultant in surgery, differ from their brothers, the physicians, in their education only, as you can easily imagine. They pay more attention to surgery than to medicine, though they generally take a medical degree. The consultant, especially, is very jealous of his title of "Mister." If you wish to see his indignation rise just call him "Doctor." Consulting surgeons are Fellows of the Royal College of Surgeons. Throughout the lectures each man's presence or absence at each lecture is registered, and if he does not attend a good portion his papers are not signed, unless the student can, by a few judiciously placed pennies, bribe the man who keeps this register. Class "Exams" and "Grinds," are held with regularity. As to fees, the perpetual ticket entitling the holder to all the hospital practice and medical school work throughout, in one of the best hospitals in London, is 132 guineas (\$670.56). This even does not entitle him to his degree, but he must pay, for the license of the Society of Apothecaries,

\$30 ; for the license of the Royal College of Physicians, \$76 ; for the membership of the Royal College of Surgeons, \$107 ; and on failing to pass, a portion only of the fees are returned. The high fees are somewhat offset by large scholarships. These, varying in value from \$250 to \$700, are won annually by competitive examinations, the poor student's only chance. I might add, what does not appear in the prospectuses of the London hospital medical schools, but which forms, to the student at least, an important part of the curriculum, there are boat-racing, football, cricket, and lawn tennis. It might be well to add to the curricula of the medical schools of the United States from the London schools, botany, forensic medicine, comparative anatomy, and mental diseases. It is not necessary to add boat-racing, cricket, etc. It would be well were the required time of medical study in the United States, instead of three years, four years as in London. It would be better were it also made a requirement that the students study with as much diligence throughout the four years as they do throughout the three. Occasionally, an Englishman goes abroad to study medicine, but not often, for there is a prevailing opinion here that what is not known in London is not worth traveling expenses. As an example of this opinion, a medical man of Edinburgh inaugurates a new system and secures its introduction and practice in Germany, France, America and elsewhere, yet is obliged to pack up his household plunder and come down to London, and become of London, before he can introduce his system in London. There is, however, at Oxford a scholarship, the taker of which must have spent eighteen months abroad.

Having the men now educated, we will start them into practice. The general practitioner of either medicine or surgery generally starts in pretty readily. He rents a house at \$250, \$1,200 or \$2,000 a year, and commences. Rather oftener than otherwise, he is in partnership with one or two others, and the firm, if a good one, will make \$10,000 to \$30,000, less costs of medicine ; one man alone, making the half or the third of this amount. His hours are rather irregular, being liable to be called at any time, but if alone he tries to keep some hours. If in a firm some one is generally at home. He does, as a rule, his own

bookkeeping, and sends his bills out by mail quarterly or semi-annually. His fees range from 62½ cents to \$2.50, with a preponderance towards the former.

The consultant's life is quite different. He thinks not of practice, but still hangs about his hospital medical school and special hospitals. He takes such positions as casualty physician, demonstrator of anatomy, practical surgery, tutors students, assists others, takes the places of members of the staff when absent, maybe gets a lectureship in the hospital medical school, next a situation as assistant physician or surgeon—his work in the out-patients' departments of the hospital. All this time his profession has brought him but the smallest stipend. After having been assistant physician to the hospital for probably fifteen years, some one above him dies, gets old, or angry, and he steps into his place. A consultant in London must hold a hospital position or his profession will amount to *nil*. During those years of trial—the first fifteen—he must keep up appearances, and seem, at least, to be making money. Many make something at other employments; for instance, writing articles for journals. Many have a private fortune to begin with, and their income keeps them. These terrible fifteen years he waits in his house, the best he can afford, and generally situated in the West End, and does not make \$500 per annum. The surgeon generally works in sooner than the physician, for people would as willingly, perhaps more so, see a young man with good nerves coming at them, knife in hand, as an old, gray-headed man with glasses, and hands like an aspen leaf. The old surgeon often throws away the knife, and only gives opinions. A consultant always charges a guinea fee,—“a guinea or nothing.” A few of the leaders, in order to “weed” a large practice, charge two guineas, and it has the desired effect. The consultant's fee is cash. He is called in, his opinion given, and the guinea taken. Such is the custom, and it is rarely broken. He keeps no books, collects no fees, writes no duns. The consulting physician is bound by his diploma not to go into partnership, nor into court for fees. Upon doing so he relinquishes his diploma, also his standing; hence, he never does it. Of this the laity, of course, are not generally aware. The consulting surgeon is really under no such restrictions; yet practi-

cally, from the general opinion of his fellows, he is as closely bound to the same rule as his brother the physician. A general practitioner of either medicine or surgery rarely grows into a consultant. In the occasions in which he does, he is expected to, as it were, wean himself from general practice by a season of total abstinence, as to practice, for two or three years. His office hours are, in the morning, until one P.M.; in the afternoon he goes out to see patients to whom he is called, usually going in a close carriage, with one horse and a coachman. The evening he spends in his study, or as seemeth to him best. In his money-making days, the consultant makes an average annual income of probably \$6,000 to \$15,000, with the leaders, who can be enumerated digitally, making \$25,000 to \$60,000 per annum. If called out of town he charges at the rate of \$3.39 per mile—no more, no less. A few of the operating fees are these: calotomy, amputation hand or arm, \$125 each; amputation leg, \$250; lithotomy, \$250. The universal shingle is a brass door-plate, which is large proportionately to the want of knowledge on the part of the practitioner. Some men embark in dispensaries. Charitable dispensaries are generally considered honorable. Paying dispensaries are frequently a little crooked. Fees are low. For instance, the window will contain the following list of prices:

Advice with medicine.....	\$.25
Visits.....	.25
Visits for one week daily.....	1.00
Midwifery, with attendance, one week	1.25

Homœopaths thrive. There are not many, but what there are are well kept by the pampered class, the nobility, who can't bear to take nasty things. A homœopath must get regularly qualified through some of the qualifying institutions. One may make loud assertions that he intends to practice homœopathically, and present himself and get his qualification from a regular licensing body. There is a homœopathic school in London, but it cannot qualify.

With reference to the social standing of the medical men of London, I wish to express myself with great caution. Few have received from them more kindnesses, more courtesies and more favors than I, an utter stranger in a strange land. Few have for

them a higher regard. Such being the fact, I hope and think it will keep me from doing them any injustice, or by mischievous words or hasty sentences conveying any impressions reflecting disparagingly against them.

In judging of their social status, one has carefully to bear in mind the peculiar forms of English society. Here men are classed after the most arbitrary fashion. One holds his social position not so much with reference to his merit and attainments as by certain accidents and adventitious relationships. Birth, kinship, wealth, title, station, even locality, chance surroundings do. as an American must think, decide most preposterously the social status of an individual. It is extremely difficult to fully understand this, unless one has resided in this country and closely studied its peculiar social conditions. The members of the medical profession form no exception to this idea—that all men are *not* born free and equal. A London medical friend recently stated to me that the physicians and surgeons in a certain portion of London, a large portion, are considered “pariahs, snobs, cads, kicked and cuffed by every one.” A sad picture, drawn by no amateur hand in these few words. And yet there are men practicing in that area whose attainments, worth and refinement of character are such as to entitle them to the very highest social position in any country where society is not outlined by such whimsical distinctions as rule here.

This reminds me to call your attention to the fact that about two years ago a London correspondent for a Philadelphia medical journal got into rather hot water with the profession here by making a similar statement—indeed, saying in so many words that the medical man in London was a social pariah. This was at once hotly contested by some of the brethren here, but the writer maintained his ground vigorously, and I am inclined to think successfully. Undoubtedly the physician does not rank so high, socially, as he deserves, but this even requires a qualification. If the medical man be compared solely with those who hold their positions not merely by extreme wealth, titles or birth, but by education and merit, he would rank high. A few have been knighted, and are Sir Medicus, and their wife Lady Medicus, and walk out to dinner before their less honored fellows. In

many cases, however, this knighthood is given with restrictions; for instance, that it cannot descend but by direct male line from father to son; no atavism allowed. Sir Medicus of course goes among the swells, who honor his presence, if not very heartily, at least by sufferance. He gets this title usually from having attended some member of the Royal family of this or some other kingdom, empire or principality in severe illness. Of course merit is presumable on his being chosen to attend in said illness. One London surgeon received his title for passing a catheter on the King of the Belgians. There are now six Sir Medici in London. The first medical man so honored was Sir Hans Sloane, in 1716. They are distinguished from the plebeians by wearing a little red band on their coat sleeves, or when out riding on their carriages. This is the symbol of all knights created since the union of the kingdoms. A created knight, too, of course is not on the same equality as one whose line runs from him who supplied James I with forty men armed and equipped, or with £1,000 to subdue the Ulster revolt, and thus getting his knighthood; not even with the batch created by Charles I to settle and mobilize Nova Scotia, which they took care not to do. Some claim that the medical man here in his various divisions ranks socially in the same scale with the various divisions of the legal profession. Many others say not; but that the rank is: clergy, law, medicine.

I am sure that the London medical profession in America would occupy a first rank, socially compared, in the eyes of his fellow men. The American medical man is much ahead of his London brother. The same is the case in Dublin and Edinburgh. In these two cities the medical man is often a social light compared to the London man. This can be explained somewhat by the fact of London being the largest and greatest city in the world, and of course the greatest men of all classes, to an extent, come here—while in Dublin and Edinburgh the medical men have not such strong competitors usually as in London. But then it is also true that London has the greatest, at least many of the greatest, lights in the medical profession.

In illustration of the position London medical men hold among their fellows, one fact. During the recent long pending of a

bill for the abolition of vivisection, in Parliament, the entire medical profession of the United Kingdom, by petitions, by deputations, arguments, labored articles in the medical journals : by importuning of members, etc., etc., brought all the force it could command to prevent the passage of said bill. Their efforts resulted in a most humiliating failure. During the pending of a bill of like character in the Pennsylvania Legislature, two medical men from Philadelphia visited the Legislature, and by a statement before it completely snuffed out the whole affair.

Their compensation shows them to be held in low estimation by their fellow men. A gentleman of good practice in London, living at an expense of \$5,000 per annum, will often furnish advice and medicine for 25 to 35 cents. Scores of highly competent medical men in London are struggling and will continue to struggle for years, scarcely able to keep the wolf from the door. It is simply impossible that a man in this condition should occupy a high social position. He could scarcely be considered other than a "pariah from society," which society makes a god of wealth, and bows with obsequious adoration before its idols of gold and of silver. Sometimes the long lane turns, if health and pluck endure. One of the most eminent men now in London scarcely earned his salt until he had passed his fortieth year. The case is different now. He is one of those who "weeds" his practice. A proper appreciation of the services of a doctor seems to be absent. The other day a "gentleman,"—the word is used in its most flexible sense—drove up to a surgeon's door with liveried driver and lackey, entered the office with a flourish, and presented an injured joint to be examined. This was done, and advice given, when this apparent gentleman threw five shillings (\$1.25) on the table. "Pardon me, sir," said the surgeon, "you have made a mistake. My fee is one guinea." (\$5.00). "Oh," he replied, "I never give a doctor more than that." "Well," said the surgeon, "then I shall be compelled not to receive anything." "Just as you please," was the reply, and scraping up the five shillings he coolly walked out. The surgeon had no alternative, and he got nothing.

Generally speaking, a professional man called to attend one of the nobility scarcely receives courtesies above those extended to a common tradesman, which courtesies here are not much. If

the case is not urgent he must await the leisurely movements of his patron, and attend when asked. For this delay he receives no explanation or apology, nor, however kindly treated by this patron in his sick chamber, would the physician dare to recognize him first on the street. He might be the recipient of a most brutal snub from Sir Blue Blood. Occasionally one loses a little of his sympathy for the maltreated Medicus when he sees him obsequiously bowing and scraping and fawning to a titled patron, and at another time coldly, imperiously and unsympathizingly cutting up and snapping at, yelling at and browbeating some helpless, suffering, trembling out-door hospital patient. The contrast between these two pictures is as that between a California May and a Maine December. Charge this not all to the doctor himself, but let his "raising" have its full share.

All good practitioners have probably read in their boyhood the Sunday-school story in which an errand boy in a store was asked by his former fellow tradesmen, the boot-blacks, how he liked it now being bossed about. "Oh, very well," he replied, "I pass it off on the ashman."

To these peculiarities of English society aforementioned is this much due. "Our insular condition," of which the Englishman is so fond of speaking, meaning his water-belted isle, is carried out in the life of the medical man. He has no more than a half dozen friends, and these form his world. All above him he fears; all below him he despises. He is, in fact, as hard on others as others are on him, and his social ostracism is in many instances from this reason merited. It is the most difficult thing in the world to get a half dozen medical men on the witness stand to agree on one of the most salient of medical scientific facts. I may also cite an instance where a medical man went on the stand and swore that a brother of the same hospital staff did not know or detect consumption in its plainest form, a failure to do which would have been a disgrace to his newest "clerk" or junior hospital walker. And this voluntarily.

And yet, let it be plainly understood, and remembered too, that there are no more patient, laborious, tender-hearted, thoroughly qualified men to be found anywhere than are many types of the London medical man.

EDWARD S. McKEE, M.D.

LONDON, 27th July, 1881.

ARTICLE XII.

LONDON, Aug. 9, 1881.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—

Although the present Congress will scarcely constitute an “epoch in the history of medicine,” as the more enthusiastic affirm, it has been the most notable, as well as the largest gathering of medical men that the world has ever seen. Everything has conduced to the success of the enterprise—the presence of numerous representative men in every department of medical art and science, the carefully-prepared programmes for sectional work, the admirably located and easily accessible rooms; while the bountiful hospitality of the London profession, displayed in elegant receptions, banquets, excursions, garden-parties, etc., as well as in private entertainments, afforded both stimulus and opportunity for individual discussions and arguments, from which the real advantage and benefit of such meetings accrue.

The most impressive scene was the opening meeting in St. James' Hall. The floor and gallery were filled with perhaps 3,000 medical men, among whom gray and bald heads predominated. The chair was occupied by one of the most eminent of English physicians—a worthy representative of the immortal name of Jenner. On the right of the chair lounged the good-natured, heavy-jawed, puffy-eyed heir to the throne of England, whose somewhat *blasé* mien presented a marked contrast to the manly features, erect figure and martial bearing of his illustrious brother-in-law and neighbor, the Crown Prince of Germany. Behind these sat a small, spare, rather effeminate looking man, whose mincing movements and careful toilet seemed curiously inappropriate for the famous Langenbeck, of Berlin. A striking contrast was a typical Englishman who sat near—a stout, compact figure, ruddy face, massive head, crowned with long silvery hair, keen gray eyes, thin compressed lips—agile, nervous Spencer Wells. Close by, a modest, rather shabby, insignificant looking individual, with the characteristic sandy complexion of the Scotch, was Keith. At a table on the left, an object of general attention, sat a wiry, restless figure, conspicuous by high cheek-bones,

a head tapering like a cocoanut, and a disposition to chatter constantly, the most genial scholar and scholarly genius of them all—Rudolf Virchow.

Further back, a tall, spare, muscular figure, distinguished among the gray heads by coal-black hair, with a rather austere, puritanical countenance (aside from a most unpuritanical blush at the end of the nose), and with a gravity of manner becoming his puritanical name—Jonathan Hutchinson. A massive, rather unwieldy form, with a fine head, sallow skin, and iron-gray hair and beard, arose in response to calls of Pasteur. A most impressive face—delicate features, high forehead, clear eyes and flowing white beard, added to a suavity of manner worthy of a princess' husband, distinguished Esmarch of Kiel. Near him sat a man whose name instantly rivets attention upon a face notable only for the immense expanse of brow. It seems incredible that this quiet, youthful looking individual (he does not seem forty-five) can be Volkmann, of Halle.

But I will not weary you with further enumeration of the hundred eminent men who occupied the same platform.

After a brief and sensible introduction by Sir William Jenner, delivered in a timid, clumsy way, the Prince of Wales read a few platitudes, and declared the Congress open. Then came a most eloquent practical address by the President, Sir James Paget, the delivery of which, in a beautifully clear, well-modulated voice, though marred by occasional awkward gestures, secured the close attention of the vast audience for three-quarters of an hour, after which the Congress adjourned for sectional work.

This work consisted, to an unpleasant degree, of twaddle by ambitious nonentities, who hoped to achieve immortality by appearance on the rostrum of the Congress, and who thereby wasted much valuable time in the reports of wonderful operations, in the description of wonderful apparatus, in the promulgation of wonderful views and theories. Yet several questions elicited real discussion, in which the leaders of medical and surgical practice put upon record their views of several still unsettled topics. It is flattering to our national pride, that we have produced not only the best horses, oarsmen and pedestrians of the year, but that the palm for advances in surgery has been almost unanimously

awarded to America. Lithotrity at a single sitting, involving often the use of a large catheter (29 or 30 Charrière) for evacuation, and the presence of instruments in the bladder for even two or three hours—in short, *Bigelow's* operation, received flattering testimonials from many, and marked approval from *all* speakers except Sir Henry Thompson. He objects to the use of the large catheter, which Bigelow insists is the essential feature of his operation, preferring to use a small lithotrite for the removal of the larger fragments. His friends are compelled to admit that, while this procedure may be safe and effectual in Sir Henry's practiced hand, it might be quite different in that of the average surgeon; while his enemies (and they are numerous in London) ascribe his opposition to personal jealousy of Bigelow. The latter's operation, therefore, while it may not effect, as the enthusiastic Telvan affirmed, a revolution in vesical surgery, is certainly the recognized and fashionable mode of lithotrity.

Emmett's operation for lacerated and everted cervix, although meeting far more vigorous opposition than Bigelow's, received the endorsement of all the representatives of the American gynecological school, and of several among the English. Playfair, in particular, made a terse and witty speech narrating his conversion to Emmett's views, as based upon a case which he (P.) had "tinkered" with various caustics for several years, and which Emmet cured completely by a single operation. The operation will doubtless now receive at least a fair trial by the conservative English, few of whom have hitherto deigned to test it, as I was informed personally by Mr. Wells and Dr. Bantock.

The medical and pathological sections labored for two days to determine the relations of renal disease to general vascular changes. Gull and Sutton attempted to force their "arterio-capillary fibrosis" heresy down the reluctant throats of the Germans; Geo. Johnson harped on muscular hypertrophy, as usual; Rosenstein gave an admirable *résumé* of the German views as to the pathology of the kidney; but when Grainger Stewart hove in sight with his interminable catalogue of imaginary "Bright's diseases," the writer fled. What the issue of the conflict was, I have not heard; but as the combatants were chiefly Gaels and Teutons, it is safe to assume that no conclusion was reached.

The sharpest discussion was that upon antiseptic surgery. Spencer Wells said that he had not used a drainage tube in ovariectomy during the last three years. (I have heard him say that he meditates abandoning the spray). Lawson Tait referred to thirty-six cases of unusual intraperitoneal operations—removal of gall-stone, opening of hydatids of liver, cysts of kidney, abscesses of pelvis, section for Fallopian pregnancy, etc. But one patient died. In only a few cases had he attempted to use Listerian details, which he soon abandoned as cumbrous and impracticable; sometimes positively injurious.

Keith stated that after losing two ovariectomy patients by carbolic acid poisoning, he had renounced the spray, and had since had sixty-five cases without a death.

Savory, while complimenting Lister on the improvement in the sanitary condition of King's College Hospital, referred to the fact that antiseptic surgery, in Lister's own hands, could not show results as good as those by the old plan at St. Bartholomew's.

On the other hand, Esmarch, Volkmann and Czerny related wonderful improvement in the statistics of their operations since they had adopted Lister's method. In the Samaritan Hospital here Thornton had twenty-one cases of ovariectomy, with one death—all carefully Listered; his colleague, Bantock, shows a similar record, except that he will not allow a drop of carbolic acid or other antiseptic to be used during an operation—employing simple water dressing.

But the crowning blow for antiseptic surgery was dealt by Lister himself. Attempting to reply to Keith, he said that he had always advised against the use of the spray in intra-peritoneal operations! That inasmuch as contagion occurred through fingers and instruments as much as by the air, the spray might perhaps be laid aside without much danger; that although he was not ready to take the step, he *might be in another year!* It was an aggravated case of "crawfishing." Esmarch, Volkmann & Co. looked decidedly disconcerted. It looks as if the bacteria superstition was about exploded even in Lister's own mind, and that he is simply "prospecting" for a good excuse, under cover of which he may extinguish his absurd and dangerous spray, and base the claims of antiseptic surgery where they belong.

I can not conclude without recording the brilliant success

achieved by Dr. Billings' wise and witty paper on Medical Literature. The close attention, signs of frequent approval and appreciation, and generous applause at the close, were handsomely crowned by Sir James Paget's remark from the chair, that if the Congress could show no other evidence of usefulness than the production of that paper, its existence would still be abundantly justified.

W. T. BELFIELD.

ASPIRATION IN DISTENDED GALL-BLADDER.—The time has come when the internal administration of drugs in cases of distention of the biliary passages should not be exclusively relied upon. Surgical interference should be practiced much more frequently than has been done. The advantages which aspiration possesses over other means of emptying the dilated gall-bladder, may be thus formulated:

I. The operation may be performed with *safety*, without taking particular precautions in uniting the walls of the gall-bladder with those of the abdomen.

II. The operation can therefore be done as soon as the diagnosis of the dilated gall-bladder has been made, if, from its size, there seems to be danger of rupture; or if the patient suffers much pain. Aside from these conditions, when aspiration should be resorted to without hesitation, the question presents itself whether it would not be good practice to evacuate the contents of a distended gall-bladder under all circumstances, simply to remove the superfluous bile, which, being cut off from its natural destination, is bound to be reabsorbed by the lymphatics, carried back into the circulation and produce, to a greater or lesser degree, a condition which is generally known as "cholæmia."

III. A very fine trocar, such as would be of not much value in case of simple puncture, can be employed, and by means of suction even a tenacious fluid can be removed from the gall-bladder.

IV. The insertion of a trocar or an aspirating needle is almost a painless procedure.

V. In cases of doubt as to the presence of gall-stones, a flexible probe can be passed through the canula and used as a sound.

VI. Aspiration being a safe and painless operation, it can be employed for the purpose of aiding diagnosis.—*Kretzschmar*.

Domestic Correspondence.

ARTICLE XIII.

MESSRS. EDITORS:—In the August number of the CHICAGO MEDICAL JOURNAL AND EXAMINER, page 178, is published the history of a case of "Extra Uterine Fœtation." As published by Dr. Rozencrans, the case is a very unusual one, and may fairly form the subject of criticism. Permit me to make a few remarks on it.

"Examination of this woman satisfied me that she was a subject for operation; that it would not admit of delay," etc. Yet the "operation" was postponed twenty-three days, and the woman, meantime, brought twenty miles in a wagon. This done, he then, and not till then, makes a thorough examination. "As there was evidently a considerable quantity of fluid in the abdominal cavity," he performed paracentesis abdominis one inch below the navel, drawing off two gallons of dark fluid; he then could feel a "solid substance" in the abdomen. Before making a thorough examination—before tapping—the reporter concluded "she was a subject for an operation," and had the woman removed a distance of twenty miles in a wagon. The data for coming to that conclusion, and for diagnosing an extra-uterine fœtation, are not vouchsafed us, as they might have been. Cases of this kind are obscure enough in their symptoms and signs, and we need further light on the method of diagnosis. Was not the tapping a most dangerous and unwarrantable proceeding? for he tells us, "after cutting through the walls of the abdomen, bringing to view the cyst . . . I . . . at once cut open the cyst," from which flowed the "same character fluid" as had been removed by the trocar. This proves that it was the "cyst" he tapped; that the

fluid was not in the abdominal cavity. Was there no peritoneum met "after cutting through the walls of the abdomen," and what was its condition? "I then tore away the placental mass, and proceeded to the removal of the sac." Removed the placenta! Is this the method of dealing with an extra-uterine placenta recommended by Barker, Thomas, Keith, or Spencer Wells? We think not. Where did the placenta grow? Where was the "sac?" and what were its adhesions? What was the condition of the foetus? He seems to have cut and torn as fearlessly as he would on a cadaver. Why was the right ovary removed?

"To obviate internal bleeding," he, after closing the wound, "placed two bricks on the abdomen for pressure" for half an hour. Were the bricks put on to compress the abdominal aorta? if so, they cannot do it; and to check bleeding in the pelvis they are equally ineffectual. Why close the wound until all bleeding was arrested? From the rough, yes, crude, manner of operating, no one can feel surprise at being told there was "for many days a copious discharge of putrid matter" through the drainage tube, notwithstanding the use of the carbolic spray during the operation. "I applied pounded ice constantly for the space of three weeks to the abdomen." Is it possible for any human being to endure such cold *so long* and live? What was the need of it? How was the peritoneum, of which we hear nothing from beginning to end, dealt with in closing the wound?

Was it absolutely necessary to practice catheterism for a month? Were opiates used? "The temperature of her body remained quite normal," yet she had a pulse of 100 in the morning and of 120 in the afternoon, for about three weeks. What is the explanation of this high pulse-rate? Was the temperature accurately measured two or three times daily by the clinical thermometer? With a "copious discharge of putrid matter" for many days, catheterism for a week, a pulse up to 120 daily for three weeks, and ice to the abdomen constantly for the same time, it does look as though there had been, after all, some peritonitis. Such persistent and prolonged use of ice should be enough to deprive any peritoneum of sensibility. Hence the patient would not complain of pain. The woman was seventy-eight days—a very long time—recovering. What was the cause of this? "I am

not aware that a similar operation was ever before performed in this State." We hope not, nor in any other State.

Such is a calm and fair criticism of this case, as reported. What woman, subjected to such conditions, could live after them? When scrutinized, the narrative looks, to me, improbable on the face of it, and beyond the name of Dr. Rozencrans, bears no evidence of authenticity. Is it meant for a rude hoax? Did the operator live in Southern Texas at the time? Did he go from Elgin to Texas to operate? or is he lately come from the part of Texas mentioned to reside in Elgin, Ill.?

THE METHOD OF REMOVING FOREIGN BODIES ENGAGED AT THE RIMA GLOTTIDIS.—M. Krishaber (France Med. 16-19, Dec. '80) relates four cases in which the foreign body rested on the superior surface of the vocal cords. In two cases a 50 centime piece was retained by the laryngeal ventricles in a horizontal position. One of these patients was placed upon his abdomen in recumbent posture, the index finger of the left hand passed as far as the edge of the epiglottis, and a pair of laryngeal forceps introduced, but it was impossible to seize the piece, which slipped from the grasp of the instrument. The patient was then placed in front of the operator, a pair of closed laryngeal forceps introduced below the glottic orifice, then opened, being careful to exercise pressure from below, upwards. The piece was dislodged and accidentally swallowed by the patient.

In the second case the piece was pushed out by means of a sound introduced through the tracheal opening made to save life, which was threatened by œdema. A bone was removed from the glottic orifice in the third case by means of forceps.

In the last of the four cases a piece of copper was extracted by the same procedure which failed in the first case mentioned.

Krishaber advises except in the case of infants in whom the larynx is easily reached by the finger of the operator, the performance of tracheotomy, the tamponning of the trachea and the extraction of the foreign body *per vias naturales*.

Editorial.

OUR CITY MORTALITY.—From the statistical tables furnished by the Health Department of this city, it appears that the gross mortality for July was 1,786, and for August 1,730. These figures not only indicate that the mortality for these months is as usual double that of any other two months of the year, but that their mortality is decidedly above the ordinary ratio for the same months in previous years. The increased mortality is as usual mostly from bowel affections in young children. But aside from this there were, in July, deaths from typhoid fever twenty-three, scarlet fever nineteen, diphtheria twenty-two, cerebro-spinal fever twenty-six, cerebral meningitis fifty, and small-pox fifty; and in August, from typhoid fever ninety-eight, scarlet fever fourteen, diphtheria forty-nine, cerebro-spinal fever thirty, cerebral meningitis forty-five, and small-pox 116. These figures, especially so far as they relate to typhoid fever, cerebro-spinal fever, and small-pox are extraordinary. Is the fault in the peculiarities of the season or in the sanitary condition of the city, or in both combined?

INTERNATIONAL MEDICAL CONGRESS.—So far as we have been able to learn, the recent international medical meeting in London was a very satisfactory one, both in regard to the number of delegates in attendance and the quantity and quality of work done. The arrangements appear to have been well made for transacting the proper business of meeting, while the hospitalities extended were abundant and cordial, and the number of those present from the various nations on both sides of the Atlantic was sufficient to give it a more truly international character than any

of the congresses that had preceded it. We have been on the lookout for some items concerning the important work done there by the numerous delegates representing our own city, but our looking has thus far been in vain. We must be content, therefore, until they come home and report for themselves.

CHICAGO DOCTORS.—Dr. A. H. Foster has furnished us with the following interesting statistics concerning the number of physicians and surgeons in this city, recognized as regular and in good standing by registration in the *Chicago Medical Register* for 1881. The whole number of names in the said register is 452. Of these, 234 are graduates from the regular medical colleges in Chicago; namely, eighteen from the Woman's Hospital Medical College, eighty-six from the Chicago Medical College, and 130 from the Rush Medical College. This would give one regular physician for every 1,112 inhabitants. Lest there should be some benevolent individuals who might fear this ratio indicated an inadequate supply of doctors in our city, we will state that the *Chicago City Directory* (Lakeside) for 1881, has the names of 892 persons claiming the title of "Doctor." This would indicate a doctor of some sort for every 563 inhabitants.

TRI-STATE MEDICAL SOCIETY.—This society, composed of members of the profession in Indiana, Illinois, and Kentucky, will hold its next annual meeting in the city of St. Louis, Missouri, commencing on the morning of the 25th of October, 1881, and continuing three days. We hope our brethren on the west bank of the Mississippi will not be frightened by the invasion of their territory, and that the members of the society after enjoying an unusual "good time," will all succeed in finding their way back to their respective States in safety.

IN view of the opinions lately expressed by eminent oculists, that the reading of German text is injurious to the eyes, the Bernese Government has resolved as much as possible to discourage its use, and all their official announcements and reports will henceforth be printed exclusively in Roman characters.

The Death of The President.

As we go to press, the City of Chicago is putting on the garb of mourning for the grievous loss it has sustained in the death of the Chief Magistrate of the Nation, JAMES A. GARFIELD. In common with the people of this and other countries, we feel keenly the grief which is awakened not only by the atrocious character of the wicked crime which has led to this result, nor yet only by the lofty position of the victim of the assassin's blow, but by the personal character of the noble martyr. His was a scholarly, an intellectual life, which appeals to the sympathies of all scholarly men, the world over. His was a gentle, home-loving and home-honoring life, which the heads of the families in this broad land were prompt to revere. His was a forgiving, uncomplaining and charitable spirit, such as that whose influence was evident in the counsels of the nation years after the body of Abraham Lincoln was laid in the dust. We have tears for his loss, gratitude for his example, reverence for the man who went down to the valley of the shadow of death without a reproach upon his lips for his murderer, and loyal honor for the gentleman who by this accident, becomes in his place the President of these United States.

One word as to the medical and surgical treatment of the case. Manifestly, until the professional gentlemen who have been in charge of it, shall have made up their record and presented it in full to their peers in medicine, it would

The Death of The President.

be a discourtesy to either criticise or condemn. In the high position of Drs. Agnew and Hamilton, the profession at large have a guarantee that what was done was well done and worthily. The results of the autopsy have shown that no knowledge as to the course exactly traversed by the ball, was had by those who treated the wound. This is exactly and precisely the position taken from the first by Drs. Hamilton and Agnew. When questioned and interviewed, these gentlemen have repeatedly and properly asserted that it was impossible to state what course the ball had traversed. The newspapers, with characteristic savagery, are even at this moment assuming that a grave error had been committed. It will be remembered that at the time of the interesting and valuable investigations made by Dr. Weisse, of New York, some account of which has appeared in these pages, Dr. Hamilton was asked his opinion of Dr. Weisse's theory. The reply was, that he (Dr. Hamilton) was "interested," in the same. He could not, however, state that the course of the ball had been decided by the experiments, because it was his opinion that the former "had not been determined." As for the treatment of the suppurating track which many took to be the course of the ball, that surely should have been the same in either event. It is no small tribute to the skill and wise judgment of the surgeons of the late president, that they wisely refrained, in the face of considerable popular clamor, to seek for the missile to an extent which might have proved mischievous, and, as the result shows, would have proved futile; the fact being that now when the case is ended and its hopeless features evident to all, there can be but praise for the skill which ministered to the invalid for so long a period without once placing his life or comfort in jeopardy.

Reviews and Book Notices.

ARTICLE XIV.—ON THE ANTAGONISM BETWEEN MEDICINES AND BETWEEN REMEDIES AND DISEASES. Being the Cartwright Lectures for the year 1880. By Roberts Bartholow, M.A., M.D., LL.D., Professor of Materia Medica and General Therapeutics in the Jefferson Medical College of Philadelphia; Author of a "Treatise on Materia Medica and Therapeutics," of a "Treatise on the Practice of Medicine," etc. 8vo, cloth, pp. 122. \$1.25. New York: D. Appleton & Co. 1881. Chicago: Jansen, McClurg & Co.

It has long been a received principle in medicine that the derangements caused by disease may be opposed and thereby made to disappear by other derangements caused by medicinal substances. Physiological antagonism means an opposition of action of poisonous medicinal agents, in that the effect of the one may be exactly counterbalanced by the effects of the other. But "such completeness of opposing action is as rare as exact similitude in remedies acting in the same way." The Hippocratic aphorism, "in general, diseases are cured by their contraries," remains true. The author then explains the doctrine of similars. "A little consideration must, I think, tend to the conclusion that, when a remedy acts in a similar manner to a disease, there must be an antagonism between the force of the remedy and the momentum acquired by the disease. If the actions were the same, the result of the combined impression would be an increase of the disturbance."

He establishes thus his theory on the basis of a general principle propounded by Herbert Spencer, that the result of the various natural forces acting together is rhythm in action. Bar-

tholow had read Spencer before writing the following: "I am not prepared to say, though I would remark that facts are not an invention of the mind, but have their reality in nature, only awaiting our investigation." Bartholow says: "In the medulla oblongata is situated a center of extensive reflex sensibility, and above it is an inhibitory center of reflex movements." The inference is that antagonistic remedies act on each of these respectively. "In the cardiac and respiratory mechanism we have admirable illustration of opposing forces producing order and rhythm. The movements of the vessels are regulated by a vasomotor center in the medulla," etc.

That rhythm is the law, however, must not be considered a truism, for, as Bartholow intimates a few lines further, whenever forces interfere in equal proportions they cease to be perceptible, so that only the rhythmical action strikes our senses and thus leads us to think that forces in action are always rhythmical. The perusal of the first lecture will repay any anxious student of philosophy and nature, and leave him convinced that the treatment of disease mostly consists in destroying a natural force (the disease) by another natural force (the remedy or medicinal agent.) The investigation of recorded cases of poisoning by opium, in which belladonna was given as an antidote, is masterly and does honor to the author. A practical point derived from the antagonism of morphia and atropia is their efficiency in preventing any bad result from either drug when given together, while their beneficial effect is increased.

The principle of physiological antagonism is destined to cause an important reform in therapeutics, and any one perusing the book under consideration will get many hints which may prove very valuable in practice; although some of them are startling ideas, they are the more reassuring since they come from Bartholow, and have been, most of them, the result of his careful investigations. Among these, the danger of administering chloral hydrate together with bromide of potassium, and chloral and morphia in full doses, is mentioned. The author claims, however, that an hypodermic injection of morphia, before administering an anæsthetic, is a valuable precaution. Strychnia in paralysis and in phthisis, belladonna in phthisis, and strychnia with

ergot in hæmorrhages are all highly recommended. But although comparatively small, this treatise on antagonism contains so many items of information that one could not review more than a few of them, and we must state that a large part of it is concerned with physiological antidotes to vegetable poisons,—a series of facts which every physician must take knowledge of, as it would be considered a criminal neglect not to know the value of belladonna in opium poisoning, etc. It is evident that every student and practitioner of medicine needs this book, and that so much knowledge in so small a compass is very unusual in medicine. H. D. V.

ARTICLE XV.—ON THE BILE, JAUNDICE, AND BILIOUS DISEASES. By J. Wickham Legg, F.R.C.P., Lond.; Assistant Physician to St. Bartholomew's Hospital, and Lecturer on Pathological Anatomy in the Medical School. 8vo, cloth, pp. 720. New York: D. Appleton & Co. 1880.

It will be seen from the title that this is a contribution to physiology and to chemistry, as well as to the practice of medicine, though given mostly to the latter. The subject is a vast one; the author had to follow untrodden paths at times, and the whole necessitated a prodigious amount of experiments. As the fanatical and ignorant act against vivisection had just been passed in the British Parliament, Dr. Legg addressed himself to the French Brotherhood and conducted many of his investigations at the Lyons laboratory, one of the best furnished in Europe. The author expresses himself very bitterly on this subject, calls the agitation a senseless outcry, and adds: "For whatever imperfections there may be in the method followed, the reader must blame those Manichæan teachings, which, harmful as they are, are almost as old as the human race itself, and the germs of which seem to exist everywhere, ready to burst into a fresh life, even amongst those who claim to be most orthodox and sincere in their morals and belief."

The author's experiments with the bile of various animals resulted in the fact that its components vary in quantity and quality according to the species. The bile of salt-water fish—the cod, and the turbot, for instance—contains chiefly taurocholate of

potash, while the bile of fresh-water fish held much more soda. As a whole these experiments do not seem very satisfactory, and they will not benefit students who need better sifted material. In "Summary of Knowledge as to the Physiology of the Bile," we meet with the statement that "it is uncertain if the liver be a mere filter, or if it secrete the bile itself, although the general belief of physiologists is in favor of the latter." "Whether the breaking down of the tissues, or the splitting up of the peptones from the food furnish the bile acids is not known." "During fasting the amount of bile is much decreased. Purging causes a decrease in amount of bile secreted. Vomiting is followed by an increased expulsion of bile; so also is muscular exercise."

Chapters X to XIX comprise probably the best treatise ever written on the subject of jaundice. Although this is more a symptom than a disease in itself, still any reader will admit after perusing these 188 pages that the subject is better treated in this manner, and rendered much more intelligible to students and even to practitioners.

The following remedies are valuable in the treatment of jaundice: Ferruginous, citrates, acetates of sodium and potassium, mineral waters, lemon juice, acids, both mineral and vegetable.

Chapters XIX to XXIII are devoted to acute yellow atrophy, or icterus gravis. As to its treatment, the author recommends first, a smart mercurial purge, followed by sulphate of magnesia, then large doses of quinine, with an admixture of the mineral acids; locally, a large, warm linseed poultice over the liver. Jaundice after poisoning by phosphorus, copper, mercury, arsenic, antimony, etc., is also described; and so the jaundice which often complicates fevers.

Chapter XXIX and last, treats of bilious diseases, in fourteen pages. This is less than the title of the book suggested. Most fevers come under that head in Hippocrates and Galen's writings. According to Stoll they are troubles of digestion, as diarrhœa, etc. Also, other writers bring under this head a decreased bile secretion.

"Here in England, a bilious attack means vomiting, with headache. If the word bilious means a gastric catarrh, there are still objections to its use."

This book, so far as the matters treated go, will remain a valuable contribution to the study of bile and the treatment of jaundice, and we would recommend every practitioner to get a copy, even if it were only for making references when treating icterus neonatorum, or acute yellow atrophy. H. D. V.

ARTICLE XVI.—MEDICAL ELECTRICITY: A Practical Treatise on the Applications of Electricity to Medicine and Surgery. By Roberts Bartholow, A.M., M.D., LL.D., Professor of Materia Medica and General Therapeutics in the Jefferson Medical College of Philadelphia, etc. Author of "A Practical Treatise on Materia Medica and Therapeutics," and of "A Treatise on the Practice of Medicine," etc. With ninety-six illustrations. 8vo, cloth, pp. 262. \$2.50. Philadelphia: Henry C. Lea's Son & Co., 1881. Chicago: Jansen, McClurg & Co.

This comes up to the standard of the author's other works, already so well known. Dr. Bartholow represents rationalism in medicine, and every line dropped from his pen is the expression of sound theories and practical facts. One should not expect to find electricity in medicine so highly commended as in Beard and Rockwell's large work, for this author is not a specialist, and would not perhaps deny the following statement of a contemporary: "Electricity has not as yet attained a very wide range of usefulness in medicine or surgery. The cost, the inconvenience, and the lack of knowledge have had something to do with this. And besides, the intrinsic value of the agent is undoubtedly less than was at first supposed and hoped." Yet electricity in the hands of the author has been very serviceable in the treatment of paralysis, rheumatism, many nervous diseases, and in constitutional diseases also; electricity at times acting as a tonic, and also as an alterative.

This book is well printed, the type is large and clear, and the contents are about all that a busy practitioner has time to peruse. It is also well adapted to students of medicine, and will sufficiently supplement their knowledge of electro-physics, of which so few of them know anything, resembling their preceptors in this. Although some enthusiastic minds once cherished the idea that

electricity was the source of all other forces, that it was the soul of nature, and consequently a promoter of life, these theories have faded away, and medical electricity received a great stroke at the hand of that writer who lately proved that it was mainly a paralyzing agent. However, electricity remains one of the surest ways to influence the mind, and thus exercise a beneficial effect on the body.

H. D. V.

ARTICLE XVII.—HERNIA, STRANGULATED AND REDUCIBLE.

With Cure by Subcutaneous Injections, together with Suggested and Improved Methods for Kelotomy. Also an Appendix, giving a short account of various Surgical Instruments. By Joseph H. Warren, M.D., Member of American Medical Association, etc. 8vo. cloth, pp. 280. \$3.00. With illustrations. Boston: Charles N. Thomas, 215 Tremont street. London: Sampson, Low, Marston, Searle, and Rivington. 1881.

This is certainly a *multum in parvo*, and that little consists in the author's operation, which is detailed in seventy-three pages, and is, as the profession is aware, a very promising treatment. A few quotations will convey the author's idea. A special syringe, invented by Dr. Warren, should be used, as no other hypodermic needle than his will answer the purpose. The legs of the patient being drawn up and the muscles relaxed, the hernia should be returned, then, "we pass the left middle finger up the spermatic canal until we come to the inguinal ring, and by slightly raising the end of the middle finger as above mentioned, the same is felt by the fore finger, which also helps us to indicate the exact point, and guide to insert the point of the instrument. Having ascertained that the ring is well open, and free from attachments or adhesions to the returned sac, we begin to insert the needle at the lower portion of the ring, where we feel its edges through the abdominal parietes. The needle should always enter this lower portion of the ring, as in passing obliquely upwards and backwards it is less likely to wound either column of the internal ring. Great care should be taken in inserting it through the integuments and superficial fascia, so as not to wound

the external pillar, but to enter the canal at once. The needle then should never be passed in a perpendicular direction, as there is thus danger of wounding the spermatic cord, but it should receive the necessary obliquity as soon as we feel that it has passed through the integuments. We can diagnose the position of the needle when first entering, by passing the left fore and little finger up with the invaginated scrotum upon it. When we have passed the needle though the integuments, we begin to open the valve and slowly push the needle in the direction already indicated. As the needle is thus inserted, it revolves and injects the fluid in sufficient quantities to cover well the external and internal rings. The needle is now slowly withdrawn, still injecting fluid in its backward motion. As soon as the needle is withdrawn pressure is made with the end of the fingers over the wound and rings for five or ten minutes, until the smarting and throbbing pain subsides."

A compress is now applied over the inguinal canal, and should be kept wet with cold water. The patient should stay in bed for a few days, and opiates should be administered during that time. Dr. Warren claims that all cases do well, and that was also the statement of the late Dr. Heaton, who used an infusion of white oak bark for an injecting fluid and a blunt hypodermic needle. Any one desiring to employ this treatment of hernia should read Dr. Warren's book and procure his instruments.

The reviewer believes in the correctness of the author's statements, yet would add that notwithstanding the wide advertisement which this method had in Europe as well as in America, he has not come across any statement in the medical press of France, England, Canada, nor the United States, to corroborate these wonderful cures. Of course this method of treatment is so new that positive results could not be well ascertained, although the seemingly harmlessness of the operation should open a wide field to enterprising surgeons, and it is time that it should be rescued from the hands of quacks.

H. D. V.

Summary.

VACCINATING FOR ANTHRAX ON A LARGE SCALE AND SUCCESSFULLY. By PASTEUR.

In April last, the Agricultural Society of Melun proposed to me, through its president, the Baron de la Rochette, to settle, by decisive experiments, the results announced by me in the Academy. I consented, and April 28 the following agreement was made :

1. The Agricultural Society of Melun shall place sixty sheep at the disposal of M. Pasteur.

2. Ten of these sheep shall receive no treatment.

3. Twenty-five of these sheep shall be vaccinated twice, with an interval of twelve days, two unequally attenuated viruses being used for that purpose.

4. These twenty-five sheep, as well as the twenty-five remaining, shall be inoculated with very virulent anthrax, after another interval of twelve or fifteen days.

The twenty-five unvaccinated sheep shall all die ; the twenty-five vaccinated sheep shall resist, and shall be compared ultimately with the ten sheep kept in reservation, in order to show that the vaccinations do not prevent the animals from returning to a normal state.

5. After this general inoculation of very virulent virus to the two sets of twenty-five sheep, vaccinated and not vaccinated, these fifty sheep shall remain together in the stables ; one of the series being distinguished by punching the ear of the twenty-five vaccinated sheep.

6. All those sheep which anthrax will kill shall be separately buried in an inclosed space.

7. In May, 1882, twenty-five fresh sheep, never used before in experimenting, shall graze in this space, in order to show that fresh sheep will spontaneously contract anthrax from the germs of the disease which earth-worms shall have brought from the cadavers to the surface of the ground.

8. Twenty-five more fresh sheep shall graze around this inclosed space, a few meters off, where no cadaver has ever been buried, in order to show that none of them shall die from anthrax.

Addition to the preceding convention programme. — The President of the Agricultural Society of Melun having manifested a desire to have these experiments include cows, I gave my consent, remarking, however, that until now, the experimental vaccination of cows had not been carried to such an extent as that of sheep, and that consequently the case might be that such experiments might not be so encouraging as those on sheep. In any case, I expressed my gratitude to that society for placing ten cows at my disposal, in order that six might be vaccinated and four left alone, and that after this vaccination the ten cows should be inoculated at the same time as the fifty sheep with most virulent virus. I affirmed, on one hand, that the six cows previously vaccinated would not be sick, while the four unvaccinated cows should perish partly or in a whole, or at least would be very sick.

I own such a programme showed a boldness in prophesying that only a brilliant success could warrant. The experiments were begun May 5, in the commune of Pouilly-le-Fort, near Melun, on a farm belonging to M. Rossignol.

In order to satisfy the wishes of the Agricultural Society which started the experiments, it was agreed to replace two sheep by two goats, and, as no condition of age or race had been proposed, it happened that the fifty-eight sheep differed in age, sex and race. Among the ten beeves were eight cows, an ox and a bull.

On May 5, 1881, were inoculated by hypodermic injection twenty-four sheep, a goat, and six cows, five drops of an attenuated anthrax virus being injected into each animal. May 17 we re-inoculated these twenty-four sheep, the goat, and the six cows with another attenuated virus more powerful than the first.

May 31, we proceeded to vaccinate with the most virulent

virus, in order to test the efficacy of former vaccinations. In order to do that, we inoculated on one hand the thirty-one beasts just spoken of, on the other, twenty five sheep, a goat, and four cows—none of the latter having been previously vaccinated.

The most virulent virus, used May 31, was one regenerated from the germinal corpuscles of the anthrax bacteria, which I had kept in my laboratory ever since March 21, 1877. In order to insure more comparative experiments, a vaccinated animal and one not vaccinated were alternately inoculated. June 2, the results were wonderful. The twenty-four sheep and the goat which had been vaccinated, together with the six cows, were all in good health; on the contrary, twenty-one sheep and the goat, which had not been vaccinated, had already died of anthrax. The three other sheep of this series died the same day.

The non-vaccinated cows were not dead, being less liable to die from the disease than sheep; but all had large swellings about the point of inoculation. Their temperature went up three degrees. The vaccinated cows had no rise in temperature, no tumor, and no inconvenience; and this shows the vaccination of cows to have been as successful as that of sheep.

ST. ANN ASYLUM. Service of PROF. BALL.

Syphilitic Insanity.—The influence of diatheses on the evolution of mental affections is now an uncontested fact. Rheumatism and gout both give rise to real insanities, which we studied in our preceding lessons under the names of rheumatic and gouty insanity. Syphilis, as the other diatheses, can also originate insanity. You know that syphilis, after it has attacked the skin and the mucous membranes, the bones, the muscles and the cellular tissue, finally takes to the viscera, and causes well-known lesions in the various parts of the nervous system. Presently, cerebral syphilis is no longer contested, and it was masterly investigated by Prof. Fournier. Anteriorly to this writer, Dagonet had sustained and proved the existence of syphilitic insanity, of which he had met a few cases at St. Ann Asylum. In Germany, syphilitic psychoses have been investigated. We shall in turn resume our actual knowledge of that affection.

And first, to what lesion shall we refer this special form of insanity, caused by syphilis? To cerebral lesions? They are so numerous, indeed, that one is only embarrassed by the choice. To an alteration of the blood? or to an unknown cause? This point has not yet been settled.

In a syphilitic patient, at what time in the course of the disease, at what age of the diathesis, says Fournier, do the cerebral manifestations appear? . . . These may come sooner or later. Although in some cases they are manifested a few months only after the original chancre, in others they may not appear till twenty or thirty years later. The medium is about the tenth year, but the exceptions are numerous.

Why should syphilis attack the brain? Here let us note that it most generally occurs in persons whose general health has much suffered, and who present that state called venereal cachexia, or syphilitic chlorosis. Beside the constitution, there may be exciting causes; for instance, the various venereal excitements, excesses of coition following long continence, which have so great an influence over mental manifestations, which are thereby perverted and disturbed in people predisposed. Add to this a powerful and preponderant cause, alcoholic excesses. Take into consideration fatigues and excesses of intellectual work, which have made the brain a *locus minoris resistentiæ*; this is an influence easily noticed among writers, lawyers, politicians, who are much more often attacked by syphilis than the lower classes, among which it is rather rare. Finally, a last cause, which no one could deny, traumatisms, strokes, and shocks to the head.

This being admitted, let us make another query before forming a judgment. Which kind of syphilis most likely originates cerebral lesions? You are aware, gentlemen, that there are benign, common and malignant syphilis; these are specially manifested by the rapidity of their course, and cutaneous lesion. Cerebral lesions are mostly excited by syphilis of medium intensity; yet we should fear the mild forms, sometimes very insidious, and not forget that there are abortive forms in which secondary manifestations may not appear. Let us now come to the symptoms of syphilitic insanity. We will overlook the other manifestations of cerebral lesions: paralyses, aphasia, amnesia, etc.,

which sometimes precede or accompany the disease under consideration. The first intellectual accident may suddenly appear. According to Fournier, insanity is often introduced by a notable failing of the intellect; then comes the syphilitic delirium, which may present itself under three forms :

1. Syphilitic dementia.
2. Syphilitic mania or insanity.
3. General syphilitic pseudo-paralysis (Fournier).

1. *Syphilitic dementia*.—The sufferer from syphilis, after a certain period, changes his moods. Some disturbance of the intellect, and peculiarities of his behavior are noticed. One who was before polite, active, bright, witty, becomes by degrees morose, impolite, dull, numb, reticent, speechless; intellectual labor is difficult, work painful, tiresome, even unbearable, in the discharge of the common duties of life. Fournier considers loss of memory a remarkable phenomenon. For my part, gentlemen, that is a trivial phenomenon; for of the various faculties of the brain, memory is the most delicate and fallible. The judgment, on the contrary, often remains right. Syphilitic amnesia may be slow and continuous, or arise suddenly. The loss of memory is sometimes almost complete, and the patient, as do some old men, may have none but youthful recollections left, recent events being forgotten; some patients even forgetting their children. Others have forgotten the simplest rules in arithmetic; others a foreign language; others at last have lost their notion of time and distance. Add to this an absolute incoherence of ideas and actions. The patient now reaches a complete dementia; being mean, filthy, disgusting and coarse. Besides a defective moral sense, and peculiar habits, the patient is so irritable as to get mad at anything, though his anger is short. To resume, this is a premature senility of mind, differing from common old age in this, that it can be cured.

2. *Syphilitic mania or insanity*.—Syphilitic delirium, as well as dementia, may appear with or without prodroma. In some there is a stage of excitement, characterized by a feverish activity of the individual, who is restless, speaks much, and forms a quantity of projects. This soon passes into frenzy and insanity; this frenzy appears under various forms. Sometimes it is a

properly syphilitic mania, in which the patient, suffering from a true muscular delirium, breaks everything which comes in his way; his words are incoherent, there is complete insomnia, as in the excitement stage of general paralysis; this might lead into some error of diagnosis, but these acute symptoms are more marked in true mania, while there is no other differential symptom. Besides that form, lypemania, with a tendency to suicide, may appear in some subjects; 2, then hypochondria in all its forms, syphilophobia especially; 3, the delirium of persecution, though vague and confused; 4, last, and this is important, there is sometimes an impulsive delirium, especially in syphilitic epileptics. A delirium of hallucination has also been described.

3. *General syphilitic paralysis*.—You are aware, gentlemen, that some authors believe syphilis the chief cause of general paralysis, while others think it has no influence in this respect. The truth lies between the two, and Fournier admits the existence of a peculiar form of syphilis of the brain which gives rise to a general pseudo-paralysis. This is manifested by an incoherence in the intellect, both in words and writings. An ambitious frenzy is rare; patients being generally dissatisfied with themselves, and they sometimes have excesses of low melancholy, as in general paralysis. Their speech is often impeded, and they stammer from paralysis, not from a muscular contraction, as is the case in general paralysis. Besides, in syphilis there is no tremulousness of the fibrillæ of the tongue, of the upper lip, nor of the facial muscles, but there is sometimes partial paralysis, especially in one eye.

A true diagnosis can be made through complications and concomitant symptoms mainly; as cephalalgia, alopecia, gummata; but especially through partial paralyses. an important indication of syphilis of the brain.

“Strabismus, it has been said, suggests syphilis.” Finally, an exhaustive and severe inquest should be made on the body of the patient, his history looked into, and not only should his family be questioned, but especially his comrades.

Whenever the patient comes under timely treatment, the prognosis is good. Hence it is necessary to act promptly and energetically. At first the mild treatment: potassium iodide, 5

grains at first, and inunctions of mercury, without any fear of any bad results from such a course (Fournier). Alternate medication is better tolerated. In case mercury could not be borne, chloride of gold may be beneficial. Finally, one must not forget that there are bad cases in which, in spite of any treatment, syphilis results in death.

MANAGEMENT OF LABOR, WITH THE PATIENT IN THE ENGLISH POSITION. By Dr. Rendu.

Labor on the side, also called the English position, because women are delivered in that way in England, is also the manner of delivery in most of the lying-in hospitals of Switzerland and Germany, but only in primiparæ. Multiparæ are delivered on their back, in France, because the elasticity of the external genital organs protects them more against lacerations of the perineum. In fact, the chief advantage of that method is to render much less frequent lacerations of the perineum in primiparæ. One can thus survey very well its progressive distension, and that of the vulva, and consequently one can appreciate better the support which the parts require; beside, the accoucheur is in a better position to give a more efficient support.

It is no exaggeration when I state that in France nearly one-half of primiparæ suffer more or less laceration of the perineum. In Vienna's Lying-In Hospital, however, where more than 9,000 women are annually delivered, the proportion of such lacerations, in primiparæ, is six per cent.; it is ten per cent., if incisions with a protected bistoury are considered as lacerations.

But, to obtain such results, I must say that it is not sufficient at the time of birth that the mother should lie on her side, and that the perineum be more or less supported; one needs some practice beside. Thanks to the courtesy of Prof. C. Braun, and his two assistants, Drs. Pawlik and Welponner, I spent three nights in Vienna's Lying-in Hospital, where I performed twenty-one deliveries on the side, under the direction of the hospital midwives, who are very experienced in the matter.

Here is briefly the *modus operandi*: As soon as the head appears at the vulva, the woman is made to lie on her left side, her right leg being raised and held by an assistant. The

accoucheur, standing on the right of the parturient woman, passes his left hand between the woman's thighs, carrying it forward and applying it against the child's head. He supports the perineum with his right hand; but the resistance thus afforded must not be a passive one. He must, on the contrary, during each labor pain, press energetically over the sacro-coccygeal region, and pull as much integument as he can over the child's head. Meanwhile, his left hand steadies the head at the vulva and prevents its coming out under the *influence of uterine contractions*. In the interval between the pains, the head goes back, soon to return again. This forced alternate motion which the head undergoes has for its result the gradual distension and a greater elasticity of the vulva. At last, the head comes out and extension takes place. One must carefully prevent this expulsion from taking place during a uterine contraction, and let the head come out when the pain is nearly over. The perineum must be supported to the end, for the passage of the shoulders is ordinarily more dangerous to the fourchette than that of the head itself. This is the manner, writes Dr. Rendu to the *Lyon Médical*, in which I deliver primiparæ at the clinique of the Charité of Lyon, and Prof. Bouchacourt insists on that method in his lectures before the medical classes.—*Le Médecin Praticien*, March 12.

FOR 5,100 medical students in 1880 in Paris, there were only two chairs of obstetrics, one of theory and one of practice. In London and Manchester there were eighteen for 2,421 students; in Scotland, six chairs, and 1,566 students; in Austria, six chairs, and 1,505 students; in Belgium, four chairs, and 238 students; in Switzerland, three chairs, and 404 students; in Germany, twenty-one chairs, and 4,405 students; in Italy, seventeen chairs, and 3,455 students. While there is thus a chair of obstetrics to 187 students in the rest of Europe, there is but one to 2,550 students in Paris, and only one clinical service for 5,100 students.

Items.

SUGGESTED PROGRESS IN MEDICAL JURISPRUDENCE.—The *St. Louis Medical and Surgical Journal*, recently contained the following suggestions :

1. In certain cases a jury of medical men should be obtained if possible.

2. Medical experts should be summoned and paid by the State, the experts acting as associate judges, assisting the judge to get at the truth.

3. Certain cases might be referred to a commission, fashioned somewhat like the present commissions in lunacy.

4. Experts called by the court to testify, should be paid. In the State of Indiana, the Supreme Court has decided, "that, under the constitution, the State has no right to take a man's particular services without compensation, and that the giving of expert medical testimony is a particular service within the meaning of the constitution."

5. The office of coroner, with the system of coroner's juries now prevailing, should be abolished, as suggested by the President of the New York State Society in 1879, and the system in vogue in Massachusetts be adopted.

4. Life Insurance companies have accumulated a mass of information relative to causes of death, the bearings of certain conditions upon the question of insanity, or of longevity, facts proving or disproving the commission of suicide, etc., etc. At present this information, so useful in the court room, is inaccessible to the majority of physicians. Is it not possible for "medical examiners," individually or conjointly, to bring this material before this society for our edification and learning.

7. Cannot members in good standing in our society be protected against irregulars by enforcing the regulation law? If men are to be permitted to practice by license, is a license granted in some other county good in this county?

8. The few instances in which examinations of supposed invalids have been ordered by the court to be held in the presence of medical men, acting for the court, as "referees and conductors," have been appreciated by the profession interested on both sides of the case: (Proc. Med. Soc. Co. of Kings.)

CRIMINAL LUNATICS.—To place insane convicts in the same institutions, and the same apartments, with insane patients from the best families of our best citizens, is inconsistent with every dictate of propriety. If a wing for insane convicts cannot be provided in connection with the State Prison, a separate building sufficiently secure and suitable for the purpose, should be erected for their accommodation: (Report of the Managers of the New Jersey State Asylum.) The medical superintendent of the same, points out that an ordinary lunatic hospital is not constructed in such a manner as to render it suitable for the detention of persons who add determined criminal propensities to insanity, and that it would destroy its hospital character, to adapt it to that class; and he pleads for the erection of a separate asylum, meeting the requirements of security, and so organized as to supply humane treatment to the really insane, without creating any incentive to convicts to feign insanity.

MENTAL STATE OF GUTEAU.—If the reports are substantially correct, that Guiteau's uncle and two cousins were insane; that he was as a boy ill-balanced, and not to be depended upon; as a youth, irritable, full of vagaries, and without power of concentration in steady work; as a man, possessed with constantly varying extravagant delusions, for which there was no sort of reasonable foundation, with a mind always vacillating between several impracticable projects; that he made a homicidal attack upon his sister with an ax several years ago, without any real provocation; that he has pursued several women with wild plans of marriage; that he has threatened various persons upon trivial grounds; and finally that he supposed he should do the country

a great service, and obey God's direct command, if he could succeed in murdering the head of the government; or even if dissapointment, revenge, political illusions, furnished the chief motives for his act, there can be no kind of doubt that he is a lunatic with delusional insanity of so long standing as to have produced very considerable dementia in a mind of originally bad quality.

The attempt to kill President Garfield differs only in unimportant details from events with which we are made almost daily familiar through the newspapers, and which are sure to occur from time to time so long as irresponsible, insane persons, are allowed to be at liberty. Although there can never be any satisfactory definition of insanity, any absolute criterion of responsibility, or any human means of determining, in doubtful cases, how far the possible self-control had been exercised, it is difficult to avoid the conclusion that all criminals, sane or insane, should be treated from the standpoint of the safety and good of society. The insane asylum is not a safe enough place of custody for the insane of the criminal class; we really need separate criminal lunatic asylums.

It rests with us to estimate the mental condition of the criminal insane, from as nearly a judicial point of view as is possible; it remains for the legislatures and courts, and not for us, to decide what the protection of society demands: (Boston Med. and Surg. Journal.)

CADAVERIC ALKALOIDS.—The very serious question, from the medico-legal point of view, of the spontaneous development of cadaveric alkaloids and their toxic power, was first raised by M. Selmi, of Bologna. After many vicissitudes of alternate favor and discredit, this subject has been revived in France by the researches of MM. Brouardel and Boutmy; in Germany by those of M. Henseman, and in Italy by the *résumé* of all the information on the subject recently published by the original discoverer, M. Selmi. The activity of some of these substances is in no degree inferior to that of the strongest poisons. There are many distinct ptomaines, each showing special chemical and physiological characteristics: and they noted the following fact, of

which the importance is clear, that one of these ptomaines, extracted from a corpse which had been eighteen months in the Seine, had all the properties of veratrine. Huseman has taken up the study of ptomaines from another point of view. He has felt doubtful whether these substances may not account for the poisoning by alimentary substances and putrid infections. The researches of Hoppe-Seyler, Schmidt, Schmideberg, and Panum, have shown, in alimentary substances in a state of putrefaction, the presence of alkaloids analogous to ptomaines. Huseman concluded that, as a matter of fact, the majority of the accidents observed in persons who had partaken of food in a state of commencing putrefaction, are to be imputed to these alkaloids, which are of similar constitution to ptomaines: (Medical News.)

A REUTER'S telegram announces that a whole boat-load of persons out for a picnic at Warrensburg, U. S. A., who partook of lemonade, were poisoned with it; eight of them are reported in the telegram to be dead, and one hundred others in a critical condition. No indication was given of the nature of the poison, and it was very probably a mineral poison. The sources of poison in drinks of the kind are twofold, being either due to incidental impurities in the ingredients—whether acids or salts—or to the apparatus in which they are made, and the consequent production of symptoms of lead poisoning. Some time since, when public attention was directed toward the subject considerably, more or less important quantities of lead were found to be frequently present in the artificial lemonade of commerce, and in other artificial aerated drinks. It is probable in this case, however, that some much more considerable accident had occurred, and that the deaths were due to the substitution of a toxic material for that which was intended. Organic and mineral impurities are not the worst enemies which the consumers of artificial waters have to fear when these waters are not prepared from sources of the utmost purity. There is a well known case in which a shooting party, who partook freely of artificial mineral waters, were, on separating, attacked in their different homes by typhoid fever, traced by the medical officer of health to the organic impurity of the drinks which they had been consuming.

The occurrence of startling incidents such as these serve especially to direct attention to the advantages of an absolutely pure mineral water. One of the first circumstances which gave a great impulse to the popularity which Apollinaris Water has now so largely achieved and so firmly established, was the public statement of an eminent hospital physician in London, a Fellow of the Royal Society, in which he described how he had himself been suffering for many months from chronic symptoms of poisoning, of which he was unable to determine the source until, having identified the cause of his disease as being lead poisoning, he succeeded in tracing it to his habitual use of soda water in syphons. The soda water had taken up enough lead from the fittings of the syphons in which it was enclosed, frequently for long periods before use, to produce the symptoms of chronic lead poisoning.—*London Sanitary Record*.

STEPHENSON COUNTY SOCIETY OF PHYSICIANS AND SURGEONS.

—Officers: *President*, L. G. Voigt, M.D.; *Vice President*, B. H. Bradshaw, M.D.; *Secretary and Treasurer*, K. T. Stabeck, M.D. *Standing Committees*:—Surgery—B. T. Buckley, M.D.; L. A. Mease, M.D. Obstetrics—B. H. Bradshaw, M.D.; T. L. Carey, M.D. Practice of Medicine—F. W. Hance, M.D.; E. A. Carpenter, M.D. Materia Medica—K. S. Marlin, M.D.; F. A. Darling, M.D. Physiology and Pathology—C. M. Hillebrand, M.D.; C. Brundage, M.D. Dermatology—L. Stoskopf, M.D.; F. C. Winslow, M.D. Publication—B. T. Buckley, M.D.; K. T. Stabeck, M.D.; T. L. Carey, M.D.

The Society is regulated by the code of ethics of the American Medical Association, and the State Medical Society of Illinois. Regular meetings are held quarterly.

SOCIETY MEETINGS.

Chicago Medical Society—Mondays, Oct. 3-17.

West Chicago Medical Society—Mondays, Oct. 10-24.

Biological Society—Wednesday, Oct. 5.

CLINICS.

MONDAY.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Prof. Holmes; 3 p. m., Otological, by Prof. Jones.

Mercy Hospital—2 p. m., Surgical, by Prof. Andrews.

Woman's Medical College—2 p. m., Dermatological and Venereal, by Prof. Maynard; 3 p. m., Diseases of the Chest, Prof. Ingals.

TUESDAY.

Rush Medical College—3 p. m., Dermatological and Venereal, by Prof. Hyde.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Quine.

WEDNESDAY.

Chicago Medical College—2 p. m., Eye and Ear, by Prof. Jones.

Rush Medical College—2 p. m., Medical, by Dr. Bridge; 3 p. m., Ophthalmological and Otological, by Prof. Holmes; 3:30 to 4:30 p. m., Diseases of the Chest, by Dr. E. Fletcher Ingals.

THURSDAY.

Chicago Medical College—2 p. m., Gynæcological, by Prof. Jenks.

Rush Medical College—2 p. m., Diseases of Children, by Dr. Knox; 3 p. m., Diseases of the Nervous System, by Prof. Lyman.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Dr. Hotz.

Woman's Medical College—3 p. m., Surgical, by Prof. Owens.

FRIDAY.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Davis.

SATURDAY.

Rush Medical College—2 p. m., Surgical, by Prof. Gunn; 3 p. m., Orthopædic, by Prof. Owens.

Chicago Medical College—2 p. m., Surgical, by Prof. Isham; 3 p. m., Neurological, by Prof. Jewell.

Woman's Medical College—11 a. m., Ophthalmological, by Prof. Montgomery; 2 p. m., Gynæcological, by Prof. Fitch.

Daily Clinics, from 2 to 4 p. m., at the Central Free Dispensary, and at the South Side Dispensary.

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Original Communications.

ARTICLE I.

MEDICAL ELECTRICITY. Opening Address before the Chicago Electrical Society, October 3, 1881. By First Vice President S. V. CLEVINGER, M.D., Member of the American Electrical Society; of the American Association for the Advancement of Science; of the American Neurological Association; Trustee of the State Microscopical Society of Illinois; Physician to the South Side City Dispensary, etc., etc.

THE PROBABLE NATURE OF ELECTRICITY has been so often discussed before this society, but little need be said under that head to-night. Sir Wm. Thompson stoutly affirms that "electricity in motion is heat;" Symmer and Du Fay regard it as consisting of two imponderable fluids permeating all ponderable matter (to my mind a most inconceivable hypothesis, as a *condition* is spoken of as a thing; a nonentity as a fluid); Tyndall, as an expounder of the laws of the correlation and conservation of forces, classifies it among the modes of motion, as a property of matter. A mental picture of energy in general may be made by conceiving *sound* to consist of motion of any substance with a

rapidity between 16 and 40,000 vibrations per second, as the ear does not take cognizance of vibrations slower or faster than these; *light*, in round numbers, being constituted by rapid molecular movements numbering 450 billion at the red end of the spectrum, to 800 billion at the violet end; less rapid motions produce *heat*, while chemical effects are wrought by vibrations quicker than that of violet light in the invisible part of the spectrum. The atomic motions known as *electricity* are immensely more rapid than those of light, and even gravity is estimated by LaPlace to be produced by matter in motion, but inconceivably quicker than light or electrical motions, because matter is acted upon instantaneously through inter-planetary space; no calculation is possible of the time required for our sun to act upon the farthest planet. Not all the phenomena of electricity are accounted for by this assumption of a certain rate of vibration.

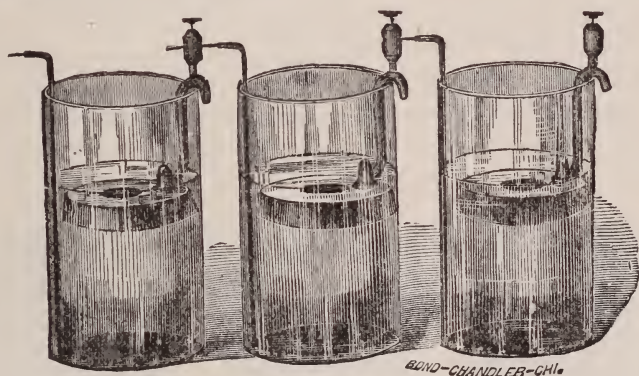
MAGNETISM can be regarded as an effect of electrization, and not as a distinct force by itself. It is in one sense a rectangular electricity, as it acts at right angles to the current causing it, and can no more be regarded as a separate energy than the attraction of gravitation can be considered apart from gravitation. Before Galvani's day the loadstone and artificial magnet were used in the treatment of disease. To-day we know the utter worthlessness of magnetism, pure and simple, as a therapeutic agent; and yet in our medical literature will be found occasional attempts at its resurrection. In surgery it has in a few instances proven useful, in aiding the extraction of foreign metallic substances from the body; as when a small piece of steel or iron had become embedded in the cornea, or had even penetrated to the lens.

I regard the advocacy of magnetism in headaches as deserving to rate with the magnetic jacket and insole nonsense we find advertised about the city.

Electrical shocks derived from the fish known as the torpedo were used ages ago as a cure for some forms of paralysis and rheumatism, but the beginning of electro-therapeutics was made 140 years since, by the suggestion of D'Helmstadt that electric sparks might be serviceable in treating diseases.

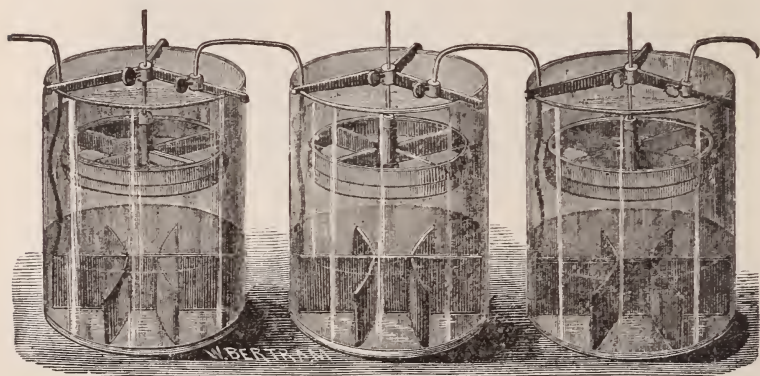
STATIC, FRANKLINIC OR FRICTIONAL ELECTRICITY was the first form to be used systematically in the cure of certain maladies.

Notwithstanding the advances made in physiological knowledge since static electricity was discovered, it has proven of but little use in medicine. Attempts have recently been made to resurrect it, and place it upon an equal footing with Galvanism and Faradization, but it does not seem probable that these attempts will succeed. Some of the great objections to the Holtz machine, used in generating static electricity, are its being cumbersome, expensive, and, even with the recent modifications, unreliable. Dr. A. D. Rockwell, an able electro-therapeutist of New York, in an article on this subject in the *Medical Record*, Sept. 17, 1881, acknowledges that the real *status* of Franklinic electricity requires more experiment and observation; that the tonic and sedative effects obtained by Franklinization are not equal to those obtained by the use of dynamic electricity, and that in electro-surgery it is manifestly of but little value.



On the other hand, he claims that in a few instances pain may be relieved by means of this apparatus, where the other machines fail; that in a few cases the treatment by sparks is preferred by the patients; that in old contractures, and possibly in cutaneous anæsthesia, Franklinization may possess some advantages. Bartholow, in his recent work on Medical Electricity, says very little about Franklinization. A few years ago an enthusiastic essay by Arthuis, of Paris, lauding static electricity as a cure-all, was translated into English in this country, but the work abounded with manifest errors, and was soon consigned to the limbo of worthless literature. At the last meeting of the Amer-

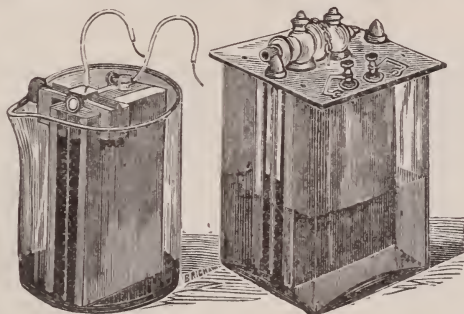
ican Neurological Society, held in New York city last June, quite a discussion was had over the probable value of Franklinic electricity in medicine. Opinions varied somewhat, but in the main it was considered as decidedly inferior to the two other forms. The methods of Charcot and Vigouroux in the use of a large platform, whereon numbers of patients were electrized at once, were characterized as unwarrantable. Dr. Morton described an apparatus, by means of which he claimed results similar to those obtained by the secondary coil, and erroneously called it a new induction current. Claims have been made that advantages are possessed by the current passing through Morton's interrupter, but this has been disputed, and Morton must experiment and report more fully before his apparatus will win favor. Dr. Onimus, one of the best electricians in Europe, refuses to use static electricity, claiming, after patient investigation, that it does no good, with the exception of affording relief temporarily in hysteria, sometimes; but this advantage not being one exclusively derivable from Franklinization, we can for the present pass over the Holtz machine and its improvements as a somewhat useless affair in treating diseases.



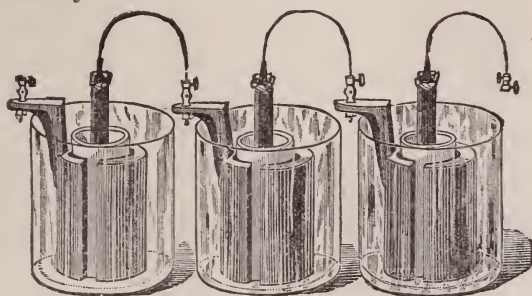
GALVANISM came into use among physicians at the beginning of this century, but it was coupled with mesmerism in the minds of the people, and falling into the hands of mountebanks, its actual worth was but little known until recently.

FARADIZATION came into vogue in 1831, and not until that date did electro-therapeutics assume a respectable appearance.

These two forms of electricity are essentially one and the same thing. An idea prevails, even among the educated, that there is an actual difference between them, as radical as between heat

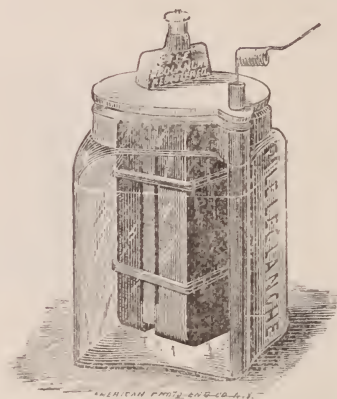
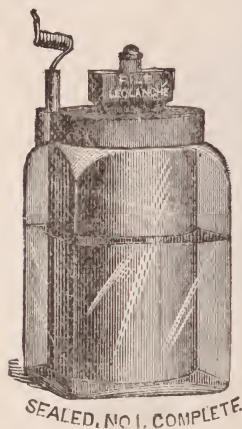


and light. Galvanic currents, by interruption with a suitable mechanism, and without the intervention of the secondary coil, can be converted into what will resemble in every respect the induced current. The Faradic is nothing more than a to-and-fro galvanic current. Its *effects*, however, as well as its uses in medicine, differ from those of the first named. This address not being intended as an elementary treatise upon electro-therapeutics, I shall merely set before you some of the cells used in generating the constant current. One of these, the celebrated Hill battery, is the invention of a member of this society, Dr. Hill, of this city.

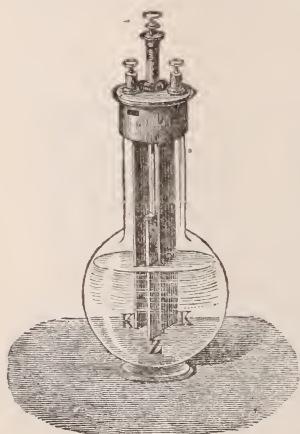


The various forms of gravity batteries here shown are serviceable for offices, but the cells of Smee and Grenet are smaller and more portable. The Grove cup is powerful, but gives off objectionable fumes. The Leclanché element is coming into use for its permanency. As Leclanché cups of two ounces can be made,

it is also portable, and being sealed, its serviceability is greatly enhanced. Dr. Hammond uses a modified Hill battery for office purposes.



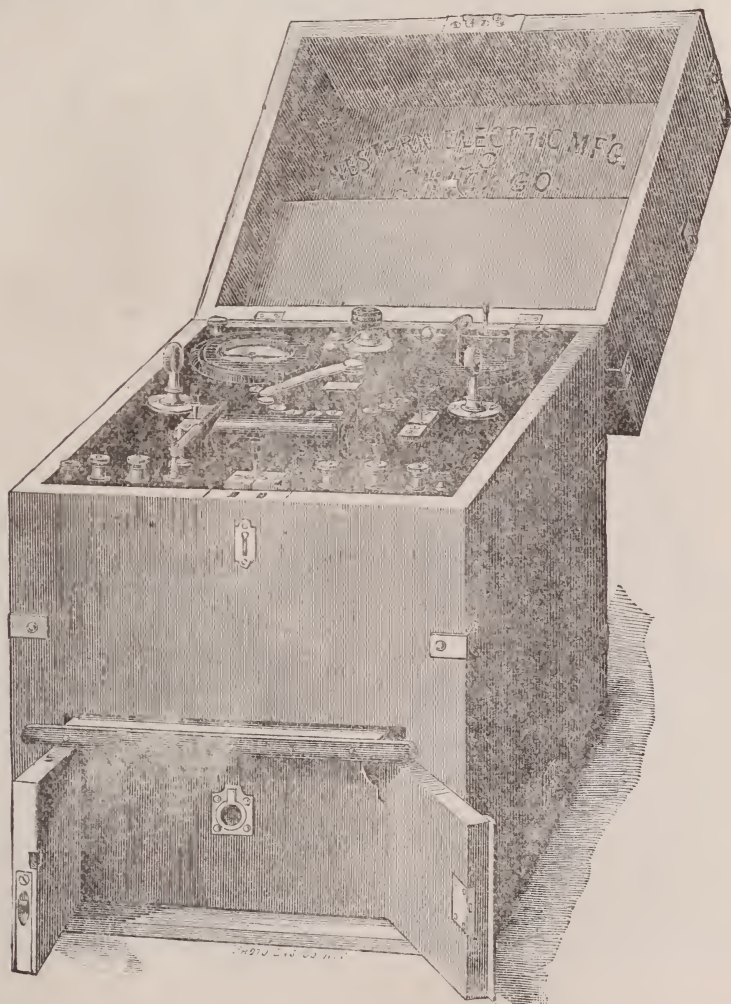
In all works on medical electricity, the Amperian method is used in designating current direction from the positive zinc plate through the solution to the negative copper plate, thence to the



positive pole attached to the copper plate, completing the circuit through the wire to the negative pole attached to the positive zinc plate. This ideal direction of the current is actually not the true one. It can be readily shown that both the negative and the positive currents (*cæteris paribus*) travel at equal rates and in opposite directions. Thus, attach a galvanometer to each pole near the battery, and a third galvanometer at the junction of the two connecting wires; upon the circuit

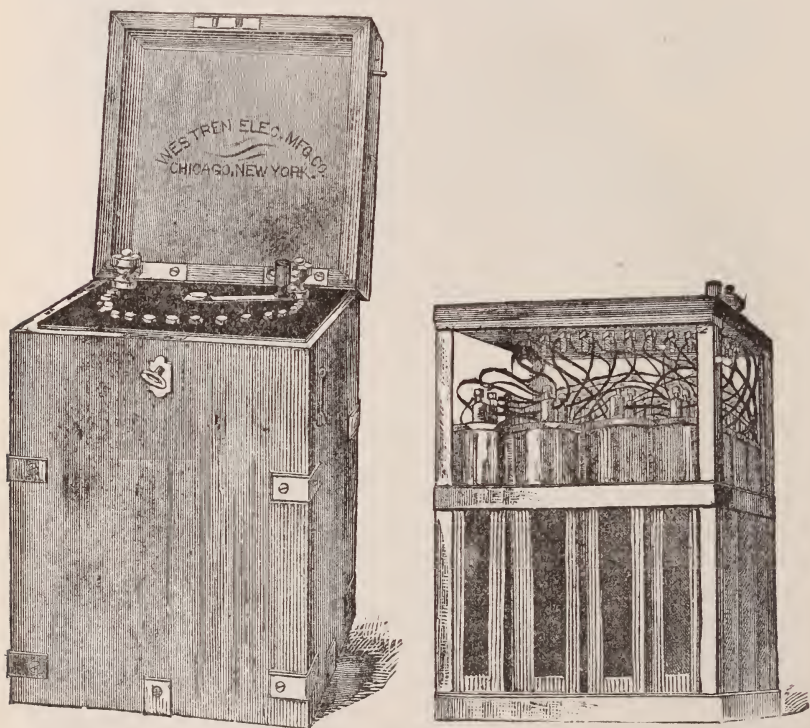
completion, the galvanometers nearest the poles will be instantaneously deflected, and the one at the junction subsequently. Designating the galvanometers in a circuit as A, B and C, if Ampere's hypothesis were true, the deflections would occur first at A, near the positive pole; next at B; then at C, next the negative pole. Instead of this we get A and C simultaneously deflected, with B lastly.

The ingenuity of medical men has been taxed severely in constructing new forms of batteries (as the entire medical machine is styled), but the main points to be considered in a portable case are power, smallness, cleanliness and simplicity.



The tip cup of Kidder has some advantages, but the Smee and Leclanché, of smaller size, are preferable. Large sized cells are of no advantage over a number of small ones, for the law of Ohm proves that although the internal battery resistance is lessened by

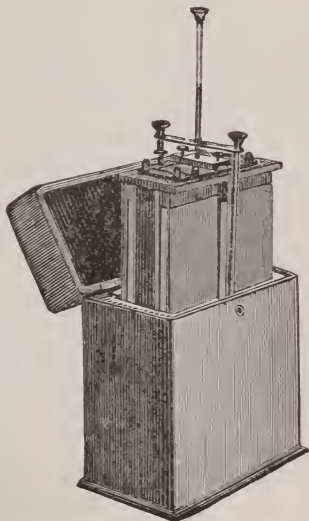
the large sized plates, the external resistance of the animal body is so very great that the quantity of electricity cannot be materially decreased unless a great number of cells be used. For the galvanic cautery, one or a few large cups are better than many small cells, the external resistance in this case being very small, as the current is only required to raise a small platinum wire to incandescence. Therefore the length of the time applied, and the number of cells used, represent the so-called dose of electricity. So at once we can see that the galvanometer, rheostat or resistance coils, are not of the slightest use in medical electricity, except for ornament, and the impression made upon the patient's mind by an array of mysterious implements.



The Galvano-Faradic Company, Dr. Kidder and Fleming and Talbot, make very good batteries, but as those of the Western Electric Company, of this city, have not been extensively figured in medical works heretofore, I exhibit that company's 32 cell

Galvanic and Chloride of Silver batteries. The first named has an electro-motive force of 60 Daniell cells; the chloride battery contains 16 or 24 cells, as desired.

This cautery battery produces a high electro-motive force with a single cell, the internal resistance of which is only .06 ohm. Four such cells as in this battery heat white hot 20 inches of No. 16 platinum wire.

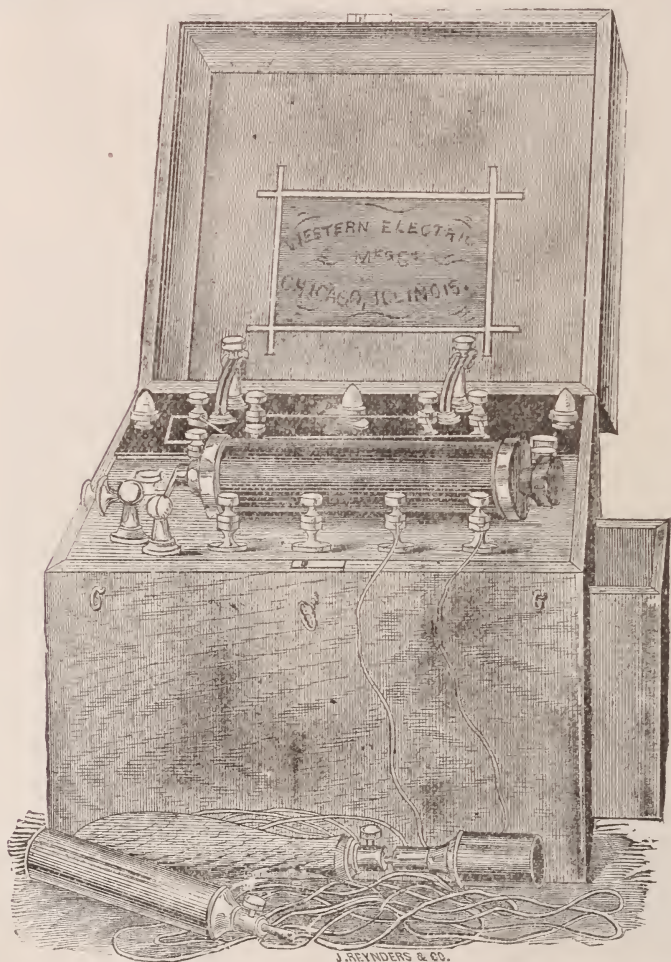


A double cell Faradaic battery is here shown, with a secondary coil resistance of 150 ohms.

A single coil battery, as well as a large number of other forms, are made by the Western Electric Company, among which we find a diminutive affair called the Pocket Induction Machine, which works an hour without re-charging. Bisulphate of mercury is the solution used, and both currents are generated. Obstetricians will find this little affair invaluable in post partum hæmorrhage.

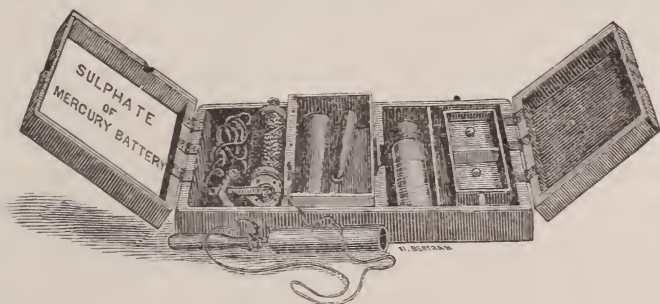
Except for surgical purposes and physiological experiment, the ordinary sponge electrodes answer the physician's purpose in most instances. A multitude of quite useless apparatus of this description has been invented, and in the majority of cases where electrodes are needed other than the sponge, they can be readily made of ordinary wire.

The Bell Induction Balance for detecting bullets, which gave unsatisfactory results in President Garfield's case, is described in the *Philadelphia Medical and Surgical Reporter* for August 13, 1881. It is a modification of the balance invented by Prof. E. D. Hughes, of London, England, a native of Bardstown, Ky. The



first practical application of this instrument was to the detection of counterfeit money, as the several alloys affect the coils differently. Geo. Hopkins, in the *Scientific American*, July 30, 1881, describes a bullet hunter on Hughes' balance principle. The

audiometer is another application, by Dr. Richardson, F.R.S., and I have been fortunate enough to secure one for exhibition. It is used for measuring the hearing, and enables the surgeon to record the progress of recovery from deafness (*vide* Report to Royal Society, *Medical Times and Gazette*, May 24, 1879).



The instrument consists of two primary coils, connected with Leclanché cells. Between these coils is a secondary coil sliding on a graduated bar. The distance between the primary and secondary coils registers the hearing capacity of the person to whose ear a telephone is applied. A microphone key interrupts the current, and the sound ceases to be heard at varying distances between the coils. The statement that an absolute zero of sound is obtainable, and the unnecessary graduation of the bar into millimeters, deserve criticism. It will prove useful to otologists. (Manufactured by Krohne & Sassmann, 8 Duke Street, Manchester Square, London, W.)

Medical electricity is divided into its applications to physiology, diagnosis, therapeutics and surgery. Galvani, Volta, Humboldt, Aldini, Du Bois-Raymond, Trowbridge, Pflüger, were prominent workers in the physiological field, and ponderous tomes have accumulated as the result of their researches. Though they added much to our knowledge, a careful revision of their labors is yet to be made in the light of a recent and better acquaintance with physiological laws. The most striking experiments in this connection were those of Fritsch and Hitzig upon the brains of dogs, and later, those of Ferrier upon that of the anthropoid ape. Definite movements of certain bodily parts took place by electrization of the bared brains of animals, and these discoveries

tended more than anything else to disparage the so-called science of phrenology, establishing in its place what is known as the localization of function in the outer part of the brain. By electro-diagnosis, in many instances, we are enabled to locate the seat of disease, and determine whether it be muscular, nervous, spinal or cerebral.

A mere enumeration of the complaints in which electricity has proven more or less useful, would be tiresome reading. Its most general application has been to paralyses where regeneration of the nerves had occurred, and where, from non-use, the muscles were atrophied; for the relief of pain, to allay spasm and cramp; in loss of sensibility; in many constitutional and local diseases. The surgical uses of this force are numerous, but afford the subject of a future lecture by another member of our society. The unsatisfactory nature of applied electricity in medicine may be realized by inspection of the excellent work of the late Dr. Charles E. Morgan, a most sincere and able worker in this field. His facilities for gathering information on these topics were the very best, and his work, entitled *Electro-Physiology and Therapeutics*, contains 708 pages, of which only 25 are devoted to therapy. He concludes this small part of his large book with the words: "Such are the definite scientific applications of electricity to medical purposes; of the many others it need only be said that they are based on incorrect theory or diagnosis of disease, or an imperfect or incorrect knowledge of electro-physiology. I do not deny that *future* researches may enable us to do more, far more, than has hitherto been done in this direction."

Take the bulk of the respectable literature on this subject, and after excluding the ordinary descriptions of apparatus and their modes of action, it is astonishing how very little there is of a practical nature in electrical treatment of disease; yet we have electric baths and other elaborate apparatus, inviting the afflicted to take a shock at from five cents on the street corner, to \$5 in some gorgeously furnished apartments. The simple little pocket battery will, in the hands of a regularly educated physician, prove infinitely more valuable in ailment treating than the most costly and imposing array of electrical apparatus, managed by an advertising ignoramus. Medical electricity does not yet deserve enough

confidence in its curative properties to warrant any one becoming a specialist in that field, nor does the possession of a fine set of electrical apparatus ensure brains or education to its owner, any more than the ownership of an amputating set creates a surgeon.

I would not have it understood that I disparage the use of electricity as a curative agent, in proper hands; but owing to the medical applications being few, and depending for their efficacy upon an intimate acquaintance with disease, it stands to reason that a very ordinary instrument, in the charge of an intelligent and regularly educated physician, will be of some service where costly apparatus, under other circumstances, may easily prove hurtful.

In this connection it may as well be mentioned that there are educated quacks, as well as ignorant ones. Here is an instance: This great volume on Medical Electricity was written conjointly by two medical men in New York. The junior partner of the firm became disgusted with the trickeries of the senior, and has, since the dissolution of the partnership, published under his own name a small work, which embodies all the essentials of electrical treatment of disease. Since Dr. Geo. M. Beard has taken to lecturing on Trance, and has been proven guilty of attempting to impose upon medical men in England with some of his mesmeric exhibitions, it is safe to presume that to him alone may be credited the pointless, voluminous nonsense of the book alluded to, and Dr. Rockwell deserves credit for what there is good in it.

There is a disposition on the part of certain medical men to resort to innumerable tricks to increase their incomes, nor do we find this class peculiar to any one so-called school. Show, pretense, glitter, owlshness, pass current too often for knowledge and skill. Seeing this, otherwise excellent physicians are tempted to resort to the same charlatanry, excusing it on the ground of having to use psychological influences on their patient's behalf. They find their clients impressed by glittering machinery, and as the imagination undoubtedly has an influence over bodily conditions, why is it not justifiable to resort to measures so harmless, that good may result? Such Jesuitical reasoning is worthy of the highway robber. It is but another step to sugar-pill placebos, and trickery of the vilest sort, all in assumed interest of the patient.

Last winter I witnessed an electrical specialist applying the

electrodes connected with a little voltaic pile to a case of acute rheumatism, and I felt perplexed when the patient said he experienced relief from pain after the application. In the first place, it is quite irrational to use electricity in this disease, and next, the current from the pile was of the weakest kind possible. The mystery was solved by the discovery that a few drops of a solution consisting of eight grains of atropine to the ounce of glycerine had been furtively applied to the painful muscles. A most powerful drug was used to deaden sensibility, while the effects were ascribed to the pile. This is psychological medicine with a vengeance.

A year or so ago, Neftel, of New York, who had published a work on the physiological effects of electricity applied to the head, was being extensively advertised by the *New York Observer* (a religious paper), as having effected marvelous cures of cataract with electricity. As Neftel had made no report of his cases to the medical journals, the genuineness of his cures was doubted, and the course of the *Observer* criticized. It was a disreputable advertising scheme.

To illustrate what the imagination will do in some cases, I shall conclude with an incident in my experience at Mercy Hospital about two years ago. A thoroughly educated gentleman, who had been a professor of chemistry in a college in this State, was suffering from spinal sclerosis, in consequence of which his arms and legs were drawn up against his body, and muscles had dwindled away seriously. Daily applications of electricity were made to special groups of muscles, after Duchenne's method, and concluded by application of the descending galvanic current to his spine. He never failed to respond to the soothing influence of the current, and every evening fell into a profound slumber after a few minutes' treatment in this way. Finally something happened to shake my belief in electricity as a hypnotic in all cases. After holding the wet sponges to his back as usual, patiently, and seeing that he had fallen asleep as he always did, I folded up the wires and turned to let down the cups from contact with the plates, and discovered that they had not been lifted at all. In fact, no current whatever had been passing. But I did not think it worth while to awaken him and tell him that he should not have gone to sleep.

ARTICLE II.

TREATMENT OF THE IRRITABLE BLADDER IN WOMEN. By
J. H. ETHERIDGE, M. D. (Read before the Chicago Gynecological Society, November, 1880.)

A too frequent desire to empty the bladder may be said to constitute what is called "Irritable Bladder." Hyperæsthesia, supersensibility of the viscus is the cause of this malady. It ought not to be confounded with irritability of the bladder, which is a physiological, not a pathological phenomenon. "Irritable Bladder," and "Hyperæsthesia of the Bladder" can be used as synonymous expressions.

The *cause* of irritable bladder determines its treatment. When it is unknown, treatment becomes guesswork. Consequently the physician's first problem of treatment is the determining of the *cause* in each individual case of vesical hyperæsthesia.

CAUSES of irritable bladder may be divided into *intrinsic* and *extrinsic* causes.

The *intrinsic causes* include abnormalities of the urine, consisting of—

1st—Too *limpid urine*.

2nd—Too *concentrated urine*.

3rd—An excess of *uric acid*, as shown by gravel, calculi, and amorphous urates.

4th—*Triple* and *amorphous phosphates*, shown in decomposition of the urine.

5th—*Oxaluria*, and

6th—*Sugar* and *albumen*.

Among *intrinsic causes* may be included abnormal substances not of urinary origin, which may be enumerated as follows :

7th—*Pus* and *blood* from renal or cystic diseases.

8th—*Feculent matter*, gall-stones, joints of tapeworms, and round worms.

9th—*Hair, fat, teeth* and *bones* from a fistulous communication with a dermoid cyst.

Other intrinsic causes include—

10th—*Cystitis, acute or chronic.*

11th—*Malignant disease of the bladder, primary or secondary.*

12th—*Polypi.*

13th—*Cysts and tubercles.*

14th—*Hypertrophy, centric or eccentric.*

Intrinsic causes may include disorders of the urethra as well as of the bladder, and are thus indicated :

15th—*Urethritis, acute and chronic.*

16th—*Neoplasm.*

17th—*Dilatation of the urethra, including that of the upper third, and of the whole canal.*

18th—*Dislocation of the urethra.*

19th—*Prolapsus of the urethral mucous membrane.*

20th—*Stricture, and*

21st—*Incomplete Fistula.*

The extrinsic causes of Irritable Bladder are numerous, often difficult to define, and are much more common than intrinsic causes. Fully two-thirds of the cases of this disorder arise from extrinsic causes. Presenting uniformly but the one symptom of frequent urination, these causes are infinitely less correctly differentiated by physicians, and consequently are worthy of the closest scrutiny and most careful management, therapeutically and otherwise. Every experienced gynecologist can recall only the many defeats in the treatment of Irritable Bladder from extrinsic causes because of not accurately ascertaining the causes in each case. These causes include :

a.—*Oöphoria, or ovarian, vascular excitement.*

b.—*Pressure on the bladder or urethra from the uterus, or from rectal abnormalities, or from intra-pelvic tumors.*

c.—*Sympathetic irritation from uterine inflammation.*

d.—*Malignant disease of the cervix uteri.*

e.—*Pregnancy.*

f.—*Vaginismus.*

g.—*Acute pelvic peritonitis and cellulitis.*

h.—*Ascarides.*

i.—*Hysteria.*

k.—*Mental troubles, fright.*

l.—Exposure during menstruation.

m.—Falls, and blows over the bladder.

n.—Masturbation and copulation ; and

o.—Malaria.

The causes herein mentioned are enumerated in extenso, for the purpose of showing that the treatment cannot be the same in all cases of irritable bladder.

The management of cases produced by causes numbered 1 to 5 inclusive, usually involves remedies addressed to nutrition. *Too limpid urine* suggests hysteria, anemia, exposure to cold, mental emotion, in short, diuresis from any cause. Treatment of these conditions involves remedies addressed to the cause whatever it may be.

Too concentrated urine, shown by the small amount voided and by high specific gravity, calls for water, simply. Ordinary drinking water, Poland water, Apollinaris water, or any of the simple mineral waters, capable of increasing the amount of water excreted by the kidneys will answer.

An excess of uric acid is one of the most common of all of the many causes of irritable bladder. Neglected, it speedily causes a congestion of the mucous membrane of the bladder, and this, in turn, propagates the polyuria and dysuria. The organic changes arising from this congestion are of a sufficiently progressive character to afford a constantly acting cause of irritable bladder. In this way, a bladder thus afflicted, at first, say, three months ago, presents *to-day* an irritable condition arising from the products of chronic congestion, whereas the excess of uric acid starting in motion this pathological train of symptoms ninety days ago, now occupies a very inferior position in the cause to-day. Consequently the majority of cases treated by physicians present not only an excess of uric acid, but they present also a vesical mucous membrane congestion. To treat this condition requires skill and patience. An excess of uric acid indicates a systemic, and, especially, an alimentary defect. This excretion, possessed of another particle of oxygen, becomes urea and ceases to be a pathological product. To supply that oxygen is no easy matter. If the blood be made to carry more oxygen and thus supply the deficiency, we must give remedies to improve the oxygenating

power of the blood. For this purpose one quarter to one grain doses of permanganate of potash, thrice daily, will be found useful. This powerful oxydising agent yields up its oxygen in the form of ozone and converts uric acid into urea. Irritable bladder relieved by the permanganate is *relieved* simply—not cured. The alkaline carbonates neutralize uric acid excess directly, and indirectly diminish it by their action on the liver. The citrate or carbonate of lithium dissolves uric acid, and is a remedy of undoubted efficacy: (Murchison, Sir Henry Thompson). Colchicum, in small doses, improves the character of the digestive ferments and promotes more perfect digestion, and in this way supplies the liver with better pabulum, thus causing a lessening of the amount of uric acid (Lauder and Brunton). Its action is surprisingly happy in very many cases—but in many other cases it seems to extend the point of tolerance, causing, unexpectedly, a vomiting and violent purging, an effect greatly to be deprecated. Fruit acids are converted into the alkaline carbonates in the blood and become dissolvers of uric acid or diuretics, and in this way are efficient aids in treating the uric acid excess. Many patients will be benefited by a hot lemonade at bedtime, or by eating a lemon before breakfast daily, or by partaking liberally of acid fruit at breakfast and lunch times. The uric acid excess thus relieved can be said to be relieved only—not cured. It is well to use them because they act quickly and satisfactorily. Thus relieved temporarily, the physician can take the necessary time to determine where the ultimate cause of uric acid excess lies and select remedies to cure it. The systemic defect is usually the ultimate cause and will be found in the primary or secondary assimilation. Constipation is almost always a conspicuous symptom in these cases. Its resultant evils include stomachic and intestinal indigestion. From it results a condition of blood poisoned with excretory material, which causes all functions of the abdominal organs to be imperfectly performed. The secreting cells of the liver fail to elaborate their products perfectly, and from this failure arises the uric acid excess. A thorough catharsis temporarily relieves all of these symptoms, and it is an exceedingly unwise thing to do to use powerful purges, because they are of only temporary benefit. The course to pursue is to

give a daily laxative, and thus by degrees purify the blood and secure a sustained functional improvement in the organs of primary assimilation. To this end, the daily use of Rakoczy, Hunyadi Janos, Friederichshall, or Victoria mineral water in warm weather; or of aloes, podophyllin, cascara sagrada, compound extract of colocynth, compound liquorice powder, or euonymus, in cold weather, will be useful. To improve the secondary assimilation we can resort to the use of bark and iron, arsenic, strychnia, minute doses of mercury when not contra-indicated, and coca. Of prime importance in these cases is the management of the diet. Farinaceous articles and acid fruits should be largely used, and only as much of the albuminous articles used as can be thoroughly digested.

Irritable bladder arising from *triple and amorphous phosphates* should be treated by treating the systemic condition producing them. They are usually found in diseases of the nerve centers and after great mental application. They generally suggest the use of rest, ergot, galvanism, massage, tonics and improvement of alimentation.

Oxaluria should be treated by paying especial attention to the "moral, mental and physical condition, and time must not be wasted in treating a mere symptom:" (Skeene). Strychnia and the mineral acids will yield the best results.

Cases involving *diabetes* or *albuminuria* call for treatment addressed exclusively to these conditions.

Irritable bladder arising from causes enumerated, 7th to 9th, inclusive, must be treated according to indication wholly, which consists in the removal from this viscus of these foreign substances.

Cases involving *cystitis, acute or chronic*, the 10th cause enumerated, are troublesome enough. Some patients will make rapid recoveries. But by far the largest majority of them will prove rebellious. The urine in these cases must be rendered alkaline as speedily as possible. Citrate of potassium, in as large doses as can be borne without causing stomachic distress, is an excellent remedy. The removal of existing constipation by daily laxative doses of mineral water upon arising mornings, is of importance, as it secures a systemic condition favorable to producing

urine of minimum acidity, after which smaller doses of the citrate of potassium will suffice to produce alkalinity of the urine. At the same time, restricting the diet to articles calculated to aid in avoiding acid excess in this excretion, should be prescribed. An exclusive milk diet has cured cases of long standing and of great severity. Alkalis, minim doses of tr. cantharides, hourly, twenty grain doses of bromide of ammonium, the solution of bromohydric acid, benzoate of ammonia in buchu, laxatives, proper diet, quietude, will relieve most cases of irritable bladder from *acute* cystitis.

The irritable condition arising from *chronic* cystitis, requires a wider range of remedial measures to meet all cases. Many women, however, are never cured, failure arising from many causes, as lack of pertinacity on the patient's part in submitting to treatment, dyscrasia, failure to apprehend and to remove co-existing disorders which sympathetically propagate the cystitis, etc., etc. Frequent urinalyses are necessary to guide us in the administration of medicines, internally. Acidity is to be modified or abolished by alkalis. Urinary decomposition calls for the sulpho-carbolates, for alyptus globulus, or for salicylate of sodium. Medicated injections into the bladder are all important in chronic cystitis. A long range of remedies is before us to select from. Antiseptic injections include solutions of common salt, potassium chlorate, carbolic acid, salicylic acid, eucalyptol, and the sulpho-carbolates. Astringent and alterative injections embrace silver nitrate, hydrastis canadensis, tannic acid, plumbic acetate, iodoform, sulphate of zinc and potassic iodide. Forceible dilatation of the urethra, self-retaining catheter and cystotomy are resorted to only after other means have failed.

Cases involving cause enumerated 11, will never be relieved permanently. Opiates or chloral may be used.

Polypi must be removed. No remedies can be resorted to in cases of too frequent urinations caused by polypi.

Cysts and tubercles are rare vesical troubles and usually cause irritable bladder. General tonics to abrogate the systemic condition producing them, and opiates to alleviate the irritable condition seem to be the remedies demanded.

Hypertrophy of the bladder, a not uncommon cause of dysuria and polyuria, must have its cause removed. Tumors and cystocele must be treated *secundum artem*; neuralgia or any functional disorder operating to cause this condition, must be considered and treated before the irritable condition can be removed.

Urethritis, acute or chronic, is a most troublesome condition to remove. The acute form generally calls for the same sort of management accorded to gonorrhœa in the male. The subacute or chronic form calls for injections, alterative and astringent. The utmost patience is necessary to *cure* cases of chronic urethritis.

Neoplasms demand surgical treatment. The general condition of the patient's health can be greatly improved by medication. Tumors or pressure in any way retarding venous circulation from the pelvis demands attention and removal if possible. Removal of the neoplasm will relieve the patient of the irritable bladder.

Cases involving causes numbering from 17 to 21 inclusive, suggest their remedies, which are surgical or mechanical.

The treatment of cases of irritable bladder produced by extrinsic causes is included in the management of those causes. The fact cannot be emphasized too much, that the most troublesome aspect of the treatment of irritable bladder in women is the ascertaining of the *cause*. The cause being determined, the principles of treatment are usually very simple.

ARTICLE III.

NEW UMBILICAL CORD REPOSITOR. By P. O'CONNELL, M.D.,
Sioux City, Iowa.

The accompanying drawing represents the tip, actual size, of an instrument intended as a repositor of prolapsed funis. This simple device is made of a smooth piece of flat whalebone, sixteen or eighteen inches long, one-fourth of an inch wide, and about half a line thick, with the sharp edges rounded off.

Point.

Shoulder.

Hole.

Actual size.



One inch from the top, a hole one-eighth of an inch diameter, is drilled through the whalebone. Half-way between the hole and the point, the edges are cut away a little, making a slight shoulder; the point is nicely rounded, yet not at all sharp.

These proportions make an instrument sufficiently strong to carry the cord up beyond the presenting part, yet flexible as well as thin enough to be left *in utero*, without doing harm, until the presentation has advanced far enough to prevent any subsequent descent of the funis; or even until the completion of the second stage of labor when it will be expelled with the child.

The instrument is quickly and easily applied thus: Take a piece of narrow tape or soft twine, at least twice the length of the whalebone; double it, and pass the double through the hole. Pass the point of the instrument *under* the funis; make a snare-loop of the doubled string passed through the hole, and slip it on the point down to the shoulder, *over* the funis, drawing the snare-loop pretty tightly on the point of the whalebone. The free ends of the tape are drawn just tight enough to hold the funis snugly without diminishing the circulation in it, and loosely wound round the instrument a few times and held from slipping through the hole while the cord is being replaced. The funis, lying on the flat surface of the instrument between the hole and shoulder, is kept there

in place by the string over it, and is then pushed up into the uterus beyond the presenting part, the whole process being completed in one or two minutes.

The slight shoulder near the top of the whalebone prevents the snare-loop on the tape running down too far and compressing the cord too much. By loosening the free ends of the tape a little, and rotating the instrument at the same time that slight traction is made on it alone, the snare-loop will slip off the point, freeing the funis, when the whalebone and tape are withdrawn from the

uterus. The advantages of this little device are, that the funis can be replaced quickly, and with certainty, and without placing the woman in the awkward genu-pectoral posture as usually advised.

Any one can make the instrument for himself, as I have done, in an hour. Whalebone of the proper width and thickness can be purchased at any dry-goods establishment, and only needs finishing.

ARTICLE IV.

ETIOLOGY OF INEBRIETY. By T. D. CROTHERS, M.D., Supt. Walnut Lodge, Hartford, Conn.

The acceptance of every new truth in medicine affecting the interest of the masses, is through the ordeal of ignorant opposition and criticism. The recognition and acceptance of the truth that insanity is a disease, was over half a century passing this period. Yet within a year the same ignorant criticism has appeared in several newspapers, urging that the insane should be held personally responsible, and punished for their acts, and not housed up and treated as sick men. The truths of the disease of inebriety are passing through the same stage. The most confusing theories, and absurd assumptions and denials, not based on any reliable studies or facts, prevail. The study of inebriety is yet in its infancy, and it is impossible from any experience or study at this time, to do more than outline some general landmarks, which stand out prominently above the wide ranges of hill and valley entirely unknown. I propose to group some of the general facts which have been noted by many observers, in the field of causes, for the purpose of showing, not how much is known, but rather the vast continent as yet unexplored, on the borders of which scientific inquiry is gathering to enter this realm, and point out the truths of this almost *Central Africa* of medical and social science.

Every case of inebriety is marked by some cerebral defect, and enfeebled will-power, with perverted nutritive functions

which begins back in some special condition or event, effecting both the structure and function of the organism. Every case will be found to follow a certain progressive march from stage to stage, with long obscure halts, or rapid strides to chronicity and death. Both exciting and predisposing causes are at work; the one may produce changes of functions and structure, which may bring on disease, or awaken latent tendencies to it; the other may induce such a state of the system inducing this disease, or weakening the power of resisting all morbid agencies which end in this malady. As in all diseases the causes all run into each other, often beginning in the organism, and in other cases in the mind, ranging over the entire field of mental pathology and closely allied with all forms of insanity. The causes divide most naturally into three classes or groups: *First.* Causes which come from inheritance, direct or indirect. *Second.* Such general causes followed by inebriety in common with insanity. *Third.* Special conditions and circumstances which favor the development of inebriety.

First.—Hereditary influence. No other disease is more certainly transmitted, not in the desire, always, for alcohol or its compounds, but in nerve degenerations and perversions, which sooner or later break out in inebriety depending on some obscure conditions. The very poor and the very wealthy furnish the most striking illustrations of this fact. The former who not only suffer from irregularities of hunger and satiety, with bad quality and conditions of food, but perverted nutrition and diminished vitality, always transmit inebriate tendencies; the latter through continuous stimulation and excess, with neglect of healthy activity of both body and mind, develop nutritive degenerations both functional and organic, which appear in the next generation in inebriety. Both occupation and mental condition at the time of procreation are active factors in the causation. Persons who are engaged in intense mental work, and leading a sedentary life, such as literary men, clergymen, bankers, book-keepers, etc., have often inebriate descendants. Inebriety not only follows from inebriate parents, but from excitable nervous persons, who have an unstable mental organization, with disordered emotional faculties, and unbalanced nutritive functions

manifest often in a capricious irregular appetite for unusual foods and fluids, etc. An inebriate diathesis can be noted in many cases, which from the absence of some special exciting causes do not always develop into inebriety. This can be studied in the practice of any physician, for there are many persons in every community who possess all the latent tendencies to inebriety; nothing but exposure to some peculiar exciting causes prevents them from being inebriates. Not unfrequently persons discover this peculiar diathesis, and avoid with great care all temptation, or conditions of surroundings which encourage the use of spirits. This diathesis is not unfrequently associated with general organic degeneration, marked by anæmia, neuralgia, affections of the heart, rheumatism, dyspepsia, tubercular deposits, and constant tendency to exhaustion, etc. The intellect is active, but uncertain, changeable, vain, sometimes with the force of a genius, and the weakness of a child, but always emotional, sensitive and sympathetic. The body and mind are developed out of proportion, unnatural special developments are present, the functional activity is intense, and they are either on the verge of eccentricity, or abject feebleness.

Second.—Such general causes from which both inebriety or insanity may follow. Of these traumatism or positive injury to the brain, or spinal cord, has been noted in many cases. These injuries are followed by alterations of nutrition, exhaustion, physical pain and depression, for which alcohol acts as a pure sedative and is taken as food, is used to relieve the cravings of hunger. Blows on the head, and injuries to the nervous trunks are often followed by inebriety. A previously temperate man falls striking on his head, or receives a blow which is soon forgotten, but he becomes a different man, and an inebriate without any reason or cause that can be seen by his friends. I have noted many cases of inebriety from railroad accidents, where the concussion and alarm were sudden, causing intense reaction and excitement, and yet no physical lesion was apparent. Injuries to distant parts of the body, react by some unknown law in inebriety. A chaplain in the late war was injured in the leg, and drank spirits to intoxication for two years until the wound healed, then became a temperate man. A physician was thrown

from his carriage, suffering from a broken arm, and became an inebriate at once. Many months after he reformed, dating from the healing of the injury. Four instances have been noted where the expulsion of a tape-worm was followed by complete recovery from the inebriety, indicating the influence of this as a cause. Other cases have been noted in which irritation from uterine troubles, and disturbances of the genital functions have appeared in inebriety. During pregnancy and lactation excessive craving for alcohol is not uncommon. There is a field of causes along the line of traumatism which, in my opinion, will throw great light on the obscure cases, and explain much that is now unknown. Conditions of degeneration and impairment of functions, either inherited or acquired may exist, which break out in inebriety either gradually or suddenly, from a train of causes that are inadequate to explain such results. The brain and nervous system has lost some power of restoration by which its integrity is preserved, or it has taken on certain conditions favorable to the development of inebriety or insanity. This may be illustrated in the physical condition following a severe attack of pneumonia; the lung power and function of respiration is defective ever after, although it cannot be defined. After an attack of typhoid fever, the patient is fully aware of impairment of force, and a want of something which existed before, although his recovery seems complete. Injury to the brain, or severe organic maladies, leave an entailment which influences the organism and mind after. Sunstroke, the toxic action of alcohol or opium, paroxysms of grief, fear, alarm, joy, may have the same effect. The inherited defects may only appear in this form; epilepsy, insanity in all its many phases, and inebriety, all indicate a brain soil differing from that of a sound person. In all these cases there is something wanting to make up cerebral health, which can only be determined by careful study in the future. I have long been convinced that the results of previous diseases were active causes in inebriety. Intermittent and malarious fevers, acute rheumatism, many of the diseases of the stomach, liver and kidneys, will be found to end in inebriety. Persistent neuralgias often break up the mind-centers, and produce exhaustion for which alcohol is found to be almost a panacea, and inebriety follows with great certainty.

Dietetic disturbances are frequently associated with the beginning of inebriety. This is apparent in the close relation of the nutritive functions and the nervous system. Dyspepsia, with all its train of perverted sensations and tastes, passes most naturally into inebriety. Excessive hunger and thirst is followed by conditions which end in inebriety. Practically any disease which breaks up the nutritive functions, causes perversions and cerebral disturbances, predisposes to inebriety in many cases. The history of inebriety is very often one of neglect of proper nutrition and general hygienic care at the beginning, then the use of beer and stronger alcohols follow. Exhaustive intellectual or physical exertion, develops either inebriety or some other form of cerebral disturbance. Scholars and students often exhibit strong tastes and longings, indicating an unbalanced mind and body, on the verge of inebriety. A prominent senator spent thirty hours in the preparation of a speech, and was an inebriate from this time; the proprietor of an ice-house tempted his workmen to continue work over forty hours continuously without much rest; two out of five men became inebriates from this occasion. A clergyman who worked excessively in a revival for many weeks was an inebriate from this time. A farmer who was overworking in the harvest field for many days drank large quantities of ice water, suffered from colic, then drank and died of inebriety two years after. Loss of property, disappointment, social or financial, grief, religious excitement, an unbalanced mind and misdirected education, are all active causes entering into the origin of inebriety. Many things in our systems of education lead directly to this end. The unphysiological methods of teaching, in bad surroundings, are the fertile soils for perverted tastes, and ignorant eccentricities from which both mental and physical dyspepsia follow; then the door is open for inebriety or some of the insanities. The dangerous ambition to sacrifice everything for wealth, power or place breaks up all healthy living, and inebriety is the result of both failure and success. Any special train of causes which break up the normal activity of the body is followed by exhaustion, and a craving for means to build up against this loss. Often this is manifest in nutrient desires, or nervous depression. For both these states alcohol in any form, either as bitters or in beer, acts as a sedative, and inebriety is thus begun.

Third.—*Special* conditions and circumstances which favor the development of inebriety, and which are noted in the majority of cases. Of these, certain periods or ages are more dangerous than others. This is founded on the physiological eras in the organization, or stages of development and decline, which have pathological relations that influence both the nervous and nutrient system. The first most dangerous period is puberty. Prolonged sexual excitement at this time is the beginning of inebriety which may not break out until full manhood. Intoxication after sexual intercourse at this period, powerfully impresses the organism with a distinct predisposition to inebriety. Later in adult life when the struggle is at its height, and all the faculties are intensely occupied, the ambitions, disappointments and changes are sudden and far-reaching; when confidence in the physical and mental powers of recovery cause recklessness of effects, then the body is extremely susceptible to inebriety and conditions which develop this malady. At this midland station between the weakness of infancy and the decline of old age, when the brain and nervous system has reached its fullest activity, both the inherited and exciting causes may concentrate in this disorder. Dr. L. D. Mason, of Fort Hamilton, N. Y., found these periods of greatest danger to be from fifteen to twenty-five, and from thirty to thirty-five.

Surroundings have a very potent influence in the production of inebriety. Mental states that give no time for change or rest, with monotonous tread-mill thoughts over and over again, as lighthouse keepers, men on the frontier, farmers far back in the interior, workmen who are closely confined to some special circle, telegraph operators, etc., where for years the same unchanging mental conditions exist. Or, on the other, between continuous change and strain to accommodate the mind to the ever-shifting scenes that appeal to the reason and emotional faculties: as, for instance, speculators, railroad and professional men, who are occupied a large part of the time, without chance for rest and change of thought. The influence of social surroundings which are degenerating and dwarfing of all the better elements of nature, are dangerous. Low society and company follows either after inebriety has begun, or is associated with a defective brain and

enfeebled moral power before alcohol is used. Domestic troubles, changing conditions of life, stimulated by pride or ambition, all help to break up the normal state of health. How far these and other circumstances of a social character enter into the causation of inebriety is not determined; but it is clear that the mind and the condition of the nervous system are very large factors in inebriety not now recognized.

The physical surroundings have a very potent influence in these cases. Temperature is often an active cause. High degrees of heat and cold may so far impair the nutrition of the nerves as to produce paresis of the vaso-motor centers, ending in inebriety. Men in profuse perspiration suddenly cooling off have become inebriates. Firemen exposed to great heat often suffer in this way. Men employed on the *South American steamers*, that ply between New York, Boston and the tropical ports, where the extremes of climate, heat and cold, alternate in rapid succession, from the frequent polar temperature of New York, to the summer tropics, soon become inebriates. Regularity and equality of food have great influence in the causation. The moment the proper nutrition of the body, both as to time and quality of food is disturbed, the soil is made ready for inebriety. Eating at irregular times, as among business and professional men who are closely engaged in absorbing work, brings on nutritive perversions; and when the character of the food is imperfect for the nourishment of the body, still further departures from health follow. If with these factors are associated unhealthy sleeping rooms, and places of business, bad air and damp unpleasant surroundings, a train of causes are at work of which inebriety is almost certain to follow. The disturbed nutrition of the body, followed by conditions of neurasthenia precedes inebriety, and every unsanitary condition intensifies and fixes this state. Certain kinds of labor, in bad surroundings and living, are also factors tending towards this malady. As, for instance, tobacco-raising in certain river bottoms in the South-west; coal mining in particular districts of England and Pennsylvania; and some kinds of manufacturing in the eastern states, etc. There are, in these, and other physical states, combinations of causes unknown at present, where inebriety, pauperism insanity, and crime often seem to flourish indigenous. There are

many forces which govern the wants of the organism, that cannot be understood to-day. The influences of the mountain, or sea air, of the effect of the winds, and degrees of heat and cold, the presence of storms and electrical changes in the air, are all noted in those who suffer from mental diseases, and among inebriates ; but how far they enter into the early causation, the student of the next century must determine. In the reports of the early insane asylums, bad temper was put down as an active cause of insanity. Now this is understood as an early symptom of disease and departure from mental health. To ascribe the first cause of inebriety to sociability, association, bad company, etc., is equally an error. These are only accidental circumstances and coincidents, which, in a certain proportion of cases, both fix and explode a long train of causes, gathering from the past and present. The causes of inebriety must be sought farther back than the statement of the patient, or his friends. His entire life must be studied, and the evidence of heredity carefully weighed, and all the circumstances of his mental, physical, social, and sexual life examined. From this only can be ascertained the causes which are formative in one of the most widespread diseases of this age. Inebriety is undoubtedly associated either as a symptom or cause of insanity in many instances, but in the vast proportion of cases it stands out alone, as a distinct disease. The complex causes which govern its march, and the obscure influences which seem to select the men of genius and talent, and those representing the other extreme of mental capacity, hurrying them on to destruction ; the pathology and the symptomatology, and the prognosis and treatment, are all in an unknown realm. Standing on this border-land, the evidences of the domain of law rich in physiological fact are apparent. When an accurate study shall clear up the fog and superstition, and inebriety and its nature will not be a question of opinion or theory, then its physical and psychical origin will be understood. Stretching out from these outline hints of causes, the medical study of inebriety gives promise of a new era in science and civilization. Some of the facts made prominent may be stated thus :

1. Inebriety is a cerebro-psychical disease, beginning in some cerebral defect in alteration of function or structure.

2. The causes are numerous and complex, blending with each

other, and depending upon many and unknown physical conditions, either inherited or acquired.

3. Its etiology must be studied from a physiological point of view, by medical men, and only from the observations of years, and the experience of many accurate observers, can it be understood.

ARTICLE V.

A CASE OF "CYANOSIS." By M. H. SEARS, M.D., Leadville, Colorado.

R. B., æt. five and one half months, was the child of parents apparently healthy. The father, a man of about thirty-three years of age, is free from tubercular or syphilitic taint, and temperate. The mother also free from taint, and previous to the birth of this child primiparous, is twenty-five years of age.

She states that the child was born at term with the aid of forceps—applied in the superior strait—after a labor of thirty-six hours duration. The child was apparently well developed, weighing about eight pounds, and made efforts to nurse during the succeeding twenty-four hours. The mother suffered from depressed nipples, and had very little milk to offer the child, so that its endeavors proved partially futile—the bottle being resorted to, to supply the deficiency. The mother, however, persevered in her endeavors to nurse her child, but at the end of about one month desisted, and resorted to the bottle entirely as a means of its subsistence. Upon this diet it seemed for a time to thrive, but finally each of the artificial "foods for infants" was tried, only to be succeeded by other diets, none of which were successful.

When the child was about two weeks old, a peculiar bluish hue was noticed over the entire integumentary surface, and particularly was it noticed when the child was excited, as in crying. Nothing was thought of this symptom for the time being, but as this "cyanotic hue" increased and became very noticeable over the scalp, it was the subject of remark.

With the appearance of the hue the digestive function partially failed, and a crop of furuncles, varying in size from one-eighth to one-half an inch in diameter made themselves manifest. These were lanced, and poured forth a quantity of semi-decomposed pus, often tinged with blood. The bowels moved from two to six times in the twenty-four hours, and presented upon examination quantities of mucus and some fecal matter,—nothing, however, of value for diagnostic use.

At the time these conditions were noted, the child was four months old, and at this time it came under my observation. I found it very much emaciated, weighing less than it did at its birth, and vomiting within a very short time after anything was introduced into the stomach.

Upon physical examination the lungs revealed the normal vesicular murmur, the præcordia normal in its boundaries, as nearly as I could judge, as the child was “pigeon breasted;” the heart-sounds normal, except the sound occurring at the pulmonic orifice; this was at times wholly suppressed. Care was taken not to confound the sound of the aortic with that of the pulmonary when the latter was present. The aortic, on the other hand, was always normal and never gave evidence of being compromised in auscultation. With these facts before me, combined with the external symptoms, I made the provisional diagnosis “cyanosis.” I immediately took steps to see if the error in the nutrition could be corrected, and prescribed a watery extract of beef prepared in the following manner:

One pound of lean beef finely chopped; one quart of cold water.

The beef was macerated in the water for six hours, strained, and the remaining liquid concentrated to one pint by boiling.

Of this extract, a teaspoonful was given every half hour. In conjunction with this diet, a powder consisting of bis. subnit. grs. v; and hyd. chlo. mite gr. ss, was given morning and evening. Under this plan of treatment the weight of the patient increased, the nausea and vomiting partially stopped, the stools became normal in color and consistency, and I had great hopes that I had adopted a plan destined to at least nourish the patient. But in time this failed, and I had to order milk, which I took

pains to see was obtained from one animal, young, new pasture fed, and well cared for.

To each quart of this milk, liq. calcis 3 ij. was added and the child allowed to nurse it from the bottle,—the milk previous to being introduced into the bottle was warmed. Nourishing enemata were also given and retained; his diet was carried out with varying success during the continuance of the case. Acid. hydroch. was tried very successfully, as an anti-emetic, in place of the bismuth salt, but its depressing action on the heart did not lead to its general use in the treatment of the case, although it proved itself fully able to check the distressing nausea and vomiting.

With these measures, the patient continued to improve slowly, but an unlooked for attack of cholera infantum aggravated all of the previous symptoms; and notwithstanding appropriate treatment was pursued to check it, the patient died in an asthenic condition. At the *post mortem* the following were the essential conditions:

The heart normal in size, general appearance, and situation; aorta, vena innominata and pulmonaries normal; ductus arteriosus impervious. An examination of the interior of the heart revealed the foramen ovale unclosed, and the Eustachian valve about as it would be in foetal life—the perviousness of the foramen being the essential pathological condition.

Another point noted was the condition of the walls of the ventricles. The wall of the right was so thin and fragile as to be torn very easily. This probably arose from partial non-use of the ventricle, on account of much of the blood passing through the foramen causing a partial atrophy. The walls of the left ventricle on the other hand were large, very thick—muscular hypertrophy from over-use; the valves normal apparently. At the time of death, child was six months old lacking five days.

THE *Atlanta Medical and Surgical Journal* will be issued in future as the “*Atlanta Medical Register*.”

H. H. DICKSON, Publisher.

ARTICLE VI.

VACCINATION: A CONSIDERATION OF SOME POINTS AS TO THE IDENTITY OF VARIOLA AND VACCINIA. By HENRY M. LYMAN, A.M., M.D., Chicago, Ill.

I have read with much interest the article under the above title, by Dr. Thomas F. Wood, in the October number of THE CHICAGO MEDICAL JOURNAL AND EXAMINER. It presents in an excellent manner the objections to the hypothesis of the identity of variola and vaccinia, reinforcing the same with a number of original observations and experiments by the author. His conclusions are essentially those which have been formulated by Professor Chauveau, and have been frequently reiterated by Dr. Henry A. Martin.

It is, in the first place, very evident that the author of the paper under consideration is somewhat embarrassed by the multitude of his unclassified facts. The meaning to be attached to the words *identity* is also a source of confusion. It should be kept in mind that in a discussion regarding scientific classifications in natural history, the words *identity* and *identical* are used in at least two ways. It is right to speak of objects as being *generically* identical, though they are *specifically* distinct and different. In the same way objects or creatures which are *specifically* identical are nevertheless differentiated into well defined varieties. Domestic sheep, for instance, are all identical in their species, but there are numerous varieties—as Merinos, South-downs, Cotswolds, etc.

Now variola presents itself as a disease which is capable of existing in numerous different species of animals, and its phenomena vary accordingly. I will not at present discuss the question whether such variations are sufficient to constitute different species, or whether the modifications should be only ranked as varieties of one species. For many reasons it will be convenient to use the term *genus* as applied to the original form of the disease. Small-pox, then, would be the generic form of the disease; horse-pox would constitute one species; cow-pox another; sheep-pox a

third; and so on indefinitely. All these different forms of disease, having one common origin, would be generically the same, though specifically different from each other. Something of this sort is what is intended when we speak of the identity of vaccinia and variola.

Dr. Wood contends, however, that small-pox and cow-pox are not thus identical; and he adduces four objections upon which his opinion is based:

1. *Because cow-pox has never been converted by any means into small-pox.*

In reply to this I would say, that the efforts of experimenters have never yet been knowingly turned in this direction. The fact that a thing has never yet been done is no proof that it cannot be done. But, according to Dr. Wood himself (p. 354), the thing has been done with great success. If his experiment at the Wilmington Small-pox Hospital was not a case of conversion of cow-pox from the cow into small-pox in the child, it is impossible to say what it was.

2. *Because, in the light of the attempts made by many careful and truthful experimenters, to produce small-pox by the inoculation of the cow with small-pox virus, the number of failures on the part of such a very large majority of them, together with the numerous positive denials made of the possibility of success, it may be concluded that small-pox virus is never converted into vaccine by transmission through the cow.*

It is very true that many experimenters in this matter have failed again and again in their efforts to variolate the cow. But it is also true that when the correct method has been employed it has yielded a successful result. Witness the interesting experiments of Mr. Badcock, the English druggist, who, after many failures, finally taught himself the proper way to variolate the cow. He made use of the best variolous virus that his physician could furnish, and his experiments were successfully repeated until he had vaccinated 14,000 children with the resulting vaccine virus, and had supplied it to not less than 4,000 physicians. Such results do not look like failure to procure vaccine virus through variolation of the cow.

3. *Cow-pox does not become epidemic, nor does it spread by*

effluvia from the human subject. It is always confined to the point of insertion in the human subject, never producing a general eruption of vaccine vesicles.

It is difficult to discover the relevancy of this proposition to the present discussion. Cow-pox never spreads by effluvia among cattle, even though it be the result of actual variolation with small-pox virus. This is one of the evidences of the modification of that virus by cultivation in the body of the cow. The statement that cow-pox never produces a general eruption of vaccine vesicles is a little too sweeping. Active vaccine virus does occasionally produce a somewhat general eruption, just enough to show its kinship to variola.

4. *Structurally considered, the vesicles of vaccination and the pustules of inoculation are very dissimilar.*

This fact cannot be urged against the identity of cow-pox. The structure of the vesicle depends upon the kind of animal on which it is developed, and upon the manner in which the virus is introduced into the body. By inoculation with variolous virus a pustule is developed on the human body which is almost identical in appearance and in its course with the results of a similar inoculation on a horse or a cow. In fact the vesicle of ordinary cow-pox is really an inoculation vesicle, for the cow seldom contracts the disease in any other way than by direct insertion of the virus, either accidentally or by design. The energy of the virus which is thus introduced is so weakened by the local processes of vesiculation that it does not produce any extensive eruption like that which often follows inoculation with small-pox virus in the human subject. This fact shows that the complete evolution of small-pox is subject to modification in accordance with the nature of the animal in which the process is carried out.

The great source of difficulty and obscurity with authors like Chauveau, Martin, and our friend Dr. Wood, lies in the fact that they do not perceive that variolous virus, like all other forms of virus, is a very mutable substance. It is continually liable to dilution and reduction of energy. It may again be vivified and made more energetic, once more to experience attenuation; and so on indefinitely. Trousseau's experiments, as related in his interesting chapter on cow-pox, are remarkably instructive. Inoculating

children with small-pox virus, and transmitting the virus from one to another in successive "cultivations," he found a continual tendency to milder manifestations of the disease in each successive crop. But occasionally there would be a reversion to the original type, and a child which had been inoculated with a mild form of the virus would be attacked with a malignant form of small-pox. The cause of such reversions was not discovered by Trousseau, and I will not now delay for its explanation. I simply wish at present to call attention to the progressive mitigation of the disease which is the ordinary result of its artificial propagation. This mitigation is still more apparent when the disease is communicated from man to the lower animals. According to the experiments of Thielé, such mitigation of the virus may be obtained without cultivation in the body of any animal whatever, by simply diluting the virus with milk after keeping it for ten or twelve days between two glasses. Dr. Wood quotes this observation (p. 350) only to discredit it. We might very easily do so, were it not that the recent experiments of Pasteur with anthrax virus render the statements of Thielé perfectly reasonable and probable. This subject, however, will soon be experimentally investigated by Burdon-Sanderson, for whose results we can wait.

Recognizing the fact that small-pox virus is capable of attenuation by successive cultivations, we can easily comprehend the manner in which such apparently irreconcilable experiments as those of Gassner, Thielé, Ceely and Badcock can be harmonized with the equally trustworthy observations of Chauveau, Wood, and others. When small-pox virus is first inoculated upon the cow, the product is a virus but one degree removed from its original intensity. If such virus be used for vaccination of the human infant, the result will either be small-pox, as Chauveau and Wood saw, or it will produce a very severe form of vaccine-disease, as was the experience of Ceely. But if this virus be still further attenuated by repeated transmissions from one cow to another, or from horse to cow, as was so common during the centuries before Jenner, the virus will be so far diluted that it will never give origin to small-pox when transplanted to the human subject. In like manner, Ceely found that the virus which he produced by

variola of the cow, though exceedingly energetic at first, even lost its dangerous energy by successive transmission from one child to another, so that after the fourth or fifth remove from the cow it was perfectly manageable, and as mild as ordinary Jennerian virus.

As for the origin of cow-pox in the cow, it is now recognized that, whatever its mediate source may be, its primitive source is small-pox. Derived from horse-pox, it only occurs when the milker of the cow is also the attendant upon a greasy horse. Sometimes, but rarely in these days, it may be derived from inhalation of air charged with variolous human excreta. When neither of these sources of contagion are active it may be traced to contact with the products of human vaccination among the milkers. Thus it is evident that, besides artificial variolation, there are at least three ways by which cow-pox may be originated in the cow. The resulting disease is characterized in every case by the same phenomena, showing that the species is the same notwithstanding the diversity of the mediate sources through which it was evolved. Cow-pox is cow-pox, no matter what may have been its immediate antecedent. But that it constitutes a species with varieties of individual manifestation is evident from the diversity of its intensity when transplanted into the human subject—a diversity which, as we have seen, is largely dependent upon the antecedent history of each specimen of the virus. Moreover, since the tendency of successive transmissions of virus through the cow is in the direction of mitigation of the successive crops, it follows that in time the product of each transmission must deteriorate just as vaccine-virus has deteriorated in the human subject. It will then be necessary to revert once more to a point nearer the original variolous source, in order to recruit the energies of our vaccine stock. This we can always accomplish in future by variolation of the cow, if Beaugency heifers cannot be discovered when needed. Guided in the preparation of such virus by the experience of the past, it will be an easy matter to avoid the difficulties and the dangers which lead men like Dr. Wood to entertain no little esteem for such a means of renewing the energy of vaccine virus.

There are many other interesting and practical thoughts suggested by consideration of this subject, but I have not time for their presentation. To those who are interested in this important matter, I can recommend perusal of Trousseau's fascinating chapter on Cow-pox, the article on Vaccination, in the seventh volume of the *Real-Encyclopädie der Gesammten Heilkunde*, the various reports on anthrax vaccination by Pasteur, Greenfield, Toussaint, and others; and, lastly, an essay on Disease Germs, by Dr. W. B. Carpenter, in the October number of *The Nineteenth Century*.

ARTICLE IV.

THE PATHOLOGY AND TREATMENT OF YELLOW FEVER; WITH SOME REMARKS UPON THE NATURE OF ITS CAUSE AND ITS PREVENTION. By H. D. Schmidt, M.D., New Orleans, La. (Concluded from page 394, October No., 1881.)

Aside from the medicines above mentioned there will, in the great majority of cases, be no further demand for others during the remaining part of the course of the disease; henceforth, the physician must concentrate his whole attention upon the study of the clinical phenomena of the case, and upon the close superintendence of the nursing of the patient; and, to accomplish this task properly, he must not limit himself to general directions, but, moreover, explain to the friends or nurse the importance of closely watching the case. He must, furthermore, take the trouble of *giving them a detailed account of the phenomena which might manifest themselves during his absence*, and what should be done in such event to meet them before he is able to see the patient himself. Yellow fever, as we have seen, is a disease of a rapid course, in which the gravest pathological changes may take place in the space of only a few days, and, if anything at all can safely be done to ameliorate the noxious effects of the poison, it must be done during the febrile stage of the disease.

With the progressive rise of the fever the patient becomes more restless, particularly from the sensation of the increased amount of heat retained within the organism, while at the same

time a delirium will gradually set in, slight at first, but generally increasing in violence with the progress of the disease. It is hardly necessary to repeat that the appearance of the delirium indicates the abnormal afflux of blood to the head, and the beginning of the hyperæmia of the brain, which, if passing beyond certain limits, will necessarily cause the death of the patient. Therefore, as soon as the temperature of the scalp commences to rise, applications of cold water should be made to the whole cranium. A towel, twice folded to obtain four layers, and *large enough to cover the whole head*, saturated with ice-water, will answer every purpose. These applications, if conscientiously made and attended to, I regard as the most important point in the whole treatment of the disease, though, if *irregularly and carelessly* applied, they will fail in their purpose. In order to avoid all interruption in their applications, there should be two towels in use, one of which to be in the ice-water always ready for application, as soon as the other is removed from the head. The most convenient method for keeping them always cold is to put a large piece of ice in a wash-basin containing just enough water to submerge the towel. In many cases it will be found that the heat of the head is so great as to warm the cold cloth in a few minutes. Though it is a difficult task to keep the head of the patient covered with the wet cloth while he is in a highly restless condition, even laboring under delirium, tossing his head from side to side, and frequently changing his position, yet *it must be done*, for if omitted during the *height* of the delirium, even for a short time, it may be followed by fatal consequences. It is most important, therefore, that at this period, the patient should never be left alone. The efficacy of these cold applications is proven by the patient becoming more quiet every time a fresh cloth is applied, and by his asking for their renewal, whenever he is in a state of consciousness—even after the febrile stage is over. Great care must be taken in making these applications not *too cold*, which would give rise to the opposite condition of the hyperæmia, a depression of the functions of the brain, no less dangerous than the congestion. For this reason, ice should not be applied directly to the head. To the extent of my observation, ice-water, if properly applied, answers all purposes.

While these ice-water applications serve to keep the hyperæmia of the brain within safe limits, they at the same time reduce the temperature of the body by abstracting a considerable amount of heat by way of the head. With the view of lowering the temperature of the body, it is recommended by many physicians to sponge with cold water, alcohol or any other rapidly evaporating liquid. As I regard the perspiration of the skin as one of the processes by which the poison is eliminated from the body, I have never adopted this practice for fear of putting a check to this process, which itself, by the evaporation taking place, can only promote the discharge of heat from the body. Besides, by indiscriminately exposing the surface of the body to the air, difficult to avoid, the blood-vessels of the skin are apt to contract suddenly and *unevenly*, driving the blood to some interior organ of the body, and increasing there the already existing congestion. It appears to me, therefore, that a cold bath of short duration would even be safer to the patient than the imperfect and unequal abstraction of heat by sponging, as the former, by acting more evenly upon the whole surface of the body, would not only more equally abstract the heat, but also prove a stimulant to the nervous system, especially if followed by a timely reaction of the skin.

For a number of years, now, the cold bath has been very extensively used in febrile diseases, especially in Germany, for the purpose of reducing the temperature of the body. This treatment is based upon the theory that the fatality of the case depends upon the abnormally augmented heat, giving rise to the disintegration of the albuminous substances, and, moreover, causing the various parenchymatous degenerations. Although the statistics taken from a large number of cases, in which the cold bath was used, compared with others of antecedent times and without the use of the bath, evidently show a considerable decrease in the mortality of these diseases, the theory of the fatality of the case depending upon the high temperature is by no means generally adopted. On the contrary, the beneficial effects of the cold bath are by many pathologists attributed to the stimulation which it affords to the nervous system; and this appears to be the true explanation.

“*Warfwinge** of Copenhagen, who not long ago investigated this subject more closely, expresses the opinion that the dangerous influence of a high temperature upon the tissues, and particularly upon the muscular element of the heart, has never been proved, and that, accordingly, the high temperature in infectious diseases is of little significance; but that the real danger consisted in an excessive formation of the specific poison of the disease. The collected 2,239 cases of typhus fever, of which 357, or minus the complicated cases, 230 terminated fatally. Only in one-eighth of these cases the temperature rose in the last days to 40° C. or higher; in the remaining seven-eighths the temperature was 39 to 40, 38 to 39, or even under 38° C. In considering the earlier stages of the disease it was found that in 52.7 per cent. of the fatal cases the temperature never reached 40°, and in two-thirds of the remaining percentage of the latter it was not higher than 40 to 40.4° only in one-sixth 41° were reached or passed. From this Warfwinge concluded that in typhus fever the degree of the degenerate processes in the tissues, as well as the derangement of the functions of the organs and the nervous system, are not at all proportionate to the height of the temperature. And, as in his opinion, it is not proved that the temperature in the interior of the body can be lowered by cold bath, he furthermore concluded that the cold baths exerted but a small influence upon the course of the disease, and should chiefly be regarded as a stimulus for the nervous system.”

Besides, there are other diseases and conditions of the organism, such as tetanus, in which the temperature of the body reaches a very high degree, without promoting the disintegration of the tissues or inducing the parenchymatous degenerations of the organs. Even in febrile diseases the disintegration of the albuminous substances commences, as indicated by the augmentation of urea in the urine, before the elevation of temperature, as has been mentioned before.

Nevertheless, cold baths of *short duration*, and repeated at intervals of time during the hot stage, may prove beneficial in the treatment of yellow fever, if used only for the purpose of

* *Virchow u. Hirsch, Jahresbericht Jour. das Jahr, 1878. Vol. I, p. 205.*

stimulating and equalizing the functions of the nervous system ; though I think that they require, in order to render them safe to the patient, more care and attention than could be generally bestowed upon the patients during an extensive epidemic, when trusty nurses are comparatively scarce. The manner in which they should be given I will quote from Liebermeister,* who has used them very extensively. It is as follows : "For adult patients the full length cold bath of 68° F., or lower, is to be preferred. The same water can be used for several successive baths for the same patient ; the bath-tub remains standing full, and the water, representing about the temperature of the room, answers the purpose without change. The duration of the bath should be about ten minutes. If prolonged beyond that it becomes unpleasant to the patient, and may even prove a damage to him. If feeble persons are much affected by the bath, remaining cold and collapsed for a long time, the duration should be reduced to seven, or even to five minutes. A short cold bath like this will have a much better effect than a longer one of lukewarm water. Immediately after the bath the patient should have rest ; he is therefore to be wrapped up in a dry sheet and put to bed (which may with advantage be warmed, especially at the foot), lightly covered, and given a glass of wine. In dealing with very feeble patients, one may begin with baths of a higher temperature, say 75°, although of course these will produce less effect. A method, especially recommended in such cases, if the surroundings permit, is that recommended by Ziemssen, of baths gradually cooled down, beginning with about 95°, and adding cold water gradually until the temperature is reduced to 72°, or below. These baths should be of longer duration."

"As a rule, in somewhat severe cases, I have the temperature taken every two hours day and night. Whenever the temperature in the rectum reaches 103°, or in the axilla 102.2°, a cold bath is given. As a matter of course, however, individual peculiarities must be taken into consideration. In children, or in persons whom one has reason to suppose capable of great resistance to the influence of heat, the temperature which calls for the bath may be placed higher, say 104° in the rectum, or 103° in

* Ziemssen, *Cyclopædia of the Practice of Medicine*. Amer. edition, vol. I, p. 208.

the axilla. In those, on the contrary, with less than the average resisting power, it may be well to employ the bath before so high a temperature has been reached, and according to the circumstances of the case, give a shorter bath, or a warmer one, or the gradually reduced bath of Ziemssen."

In following the above directions for the application of the cold bath in febrile diseases, however, it should be remembered that they were written in a cold, rigorous climate, like that of Germany; some allowance should therefore be made, on the other hand, for the semi-tropical one under which we live, where most of the people are accustomed to a certain amount of continuous heat of the weather almost throughout all seasons of the year.

It is hardly necessary to pass any remarks on the method of reducing the temperature of the body by permanently placing the patient upon a cot for the purpose of keeping up a continuous spray of cold water over the whole body,—the impracticability of which the inventor proved by paying with his own life for the unphilosophical idea of preventing the perspiration of the skin, and inducing a depression of the nervous system.

As regards the delirium I have to add that, in the epidemic of 1867, I have, in severe cases, frequently made use of a fly-blister to the nape of the neck, leaving it long enough to produce supuration. The object, as may be guessed, was to invite the blood from the congested brain to the sore. But, although this practice was generally followed by favorable results, I would not pretend to assert that they were solely due to the application of the blister, but rather suspect that the cold applications to the head, used at the same time, contributed their full share. In the succeeding epidemics I omitted the blister, and directed my attention chiefly to the proper application of the cold to the head, of which the results were equally favorable, and without giving so much trouble to the patient, as is always associated with the application of a blister in that locality.

The pain over the bladder, associated with a temporary retention of urine, frequently observed during the febrile stage, is generally relieved by the application of a warm flax-seed poultice with some laudanum poured over it, to be continued until the pain is relieved and the urine commences to flow.

The nausea and constant inclination to vomit during the febrile stage of the disease is very distressing to the patient, and difficult to allay. The application of a sinapism over the epigastrium affords sometimes relief, but not permanently. I have before remarked that the condition of the stomach is a sure index of the condition of the liver, and the latter of the condition of the whole organism; it is therefore important that the stomach should be examined from time to time, in order to ascertain whether it is sensitive to pressure upon it, or not. As long as there is no pain produced by the pressure there is no immediate danger to the patient, while, on the other hand, the production of pain by pressure indicates a highly congested condition of this organ and a considerable fatty infiltration of the liver, which possibly may be accompanied by a rupture of the minute vessels of the mucous membrane of the stomach, giving rise to black vomit. As soon, therefore, as the least tenderness is noticed, such measures as will induce a revulsion from that organ to the skin should at once be taken. In many cases a mustard cataplasm answers all purposes; in others, however, the emplastr. cantharides must be resorted to.

In order to allay the thirst of the patient, small fragments of ice may be given to be dissolved in the mouth, or even to be swallowed entire; the moderate cold thus produced upon the mucous membrane of the stomach may contribute to restore to a certain degree the tonus of the minute blood vessels. The ice, however, must be given with discrimination, as an excessive cooling down of the stomach might at the same time exert a depressing influence upon the sympathetic ganglia of the solar plexus, situated in the close vicinity of this organ. The same danger will accompany the application of solid ice to the epigastrium for the purpose of arresting the black vomit. If the latter really occur, the most reliable remedy, perhaps, is the swallowing of small pieces of ice. Iced champagne is a favorite remedy with many physicians, and I have often ordered it myself, though I would not assert that it exerted a special influence upon the blood vessels of the stomach; if followed by good results it may be presumed that they were chiefly due to the ice.

If the patient has safely passed the febrile stage, accompanied by the hyperæmia of the brain, the chief danger during the second stage, especially if the delirium was severe, lies in the great depression of the nervous system, being mostly felt by the heart, but also by other organs, as the liver and kidneys. The depression of the heart's action, however, may be increased by the degeneration of a part of its muscular elements, occurring in severe cases, and which may lead to a paresis or even paralysis of this organ. For this reason, as soon as the febrile storm has passed, the action of the heart should be closely watched, and if found too weak for the performance of its function, stimulants should at once be administered. While some physicians make use of digitalis, carbonate of ammonia, etc., which may well answer the purpose, others, perhaps the greater number, prefer the alcoholic stimulants; for my own part, I have always used the latter, as they are generally ready at hand and answer the purpose equally as well. They must, however, be administered with great caution and discrimination, as by an excessive use they may do as much harm as good, by overstimulation being followed by a worse depression, or by irritating the stomach leading to black vomit. Unfortunately, they are but too frequently misapplied to the disadvantage of the patient; from all I know, brandy and whisky flow too freely during an epidemic of yellow fever, so much so, that the people have become accustomed to regard these liquids as the chief remedy for the disease. I have learned of cases in which they have actually been given during the febrile stage of the disease, when they obviously can only increase the already existing excitement and augment the disturbance of the functions of the organs. On the other hand, however, I have met with cases, in which a tablespoonful of pure whisky, slightly diluted with water, and administered at the proper time, has evidently saved the patient's life by arousing the very much depressed action of the heart. As regards the selection of the stimulants, it may be said that while the ordinary wines, besides containing too little alcohol, are unsuitable on account of their excessive acidity and impurity, the finer sorts, like genuine old Sherry and Madeira, are too expensive and, also, difficult to be obtained. Pure brandy or whisky, therefore, will be the most preferable stimulants.

If during the course of the disease the urinary discharges are interrupted, the catheter, of course, should be used, in order to ascertain whether there is any urine in the bladder, or whether the non-appearance of this fluid is really due to a deficiency of secretion, depending upon the degeneration of the epithelium of the uriniferous tubules or the obstructions caused by the various infarctions in the latter. In those cases in which a suppression of the urinary secretion really occurs, it appears to me that nothing direct or reliable could be done to induce the kidneys to resume their functions, as the interruption depends upon the organic changes in these organs; though, if depending merely upon the infarctions of the tubules, especially the straight, these obstructions may be overcome, in some cases, and cleared by the pressure of the urine from behind them; the urine, then, will re-appear. On the whole, the stimulation of the nervous system appears to me here the proper course to be pursued; though, in such cases, the condition of the organs in general is already so low as to leave but very small chances for the recovery of the patient.

As regards the diet in yellow fever it may be said that during the febrile stage of the disease it is of little importance, for the reason that the patient generally refuses anything in the shape of food offered to him. Nevertheless, it appears to me that the stomach should not be left entirely empty during the stage, and some light nourishment in liquid form should be given. A thin soup of barley, or any other farinaceous liquid answers perhaps the purpose best; and, if given cold, may at the same time serve to allay the thirst. In the second stage, small doses of the essence of beef—always freshly prepared—or milk will answer the purpose. It is sufficiently known that some caution should be observed during the convalescence in not over-feeding the patient, an error which may be followed by very serious consequences. The diet, therefore, should be chiefly albuminous, very digestible, and given in small doses at first, until the congestion of the stomach may have entirely disappeared. But, it is not only the stomach where the danger of over-feeding lies; for, though the food may be properly digested and the nutritive matters absorbed into the blood, the degenerated and weakened organs and tissues may not be able to assimilate them, and a new disturbance in their nutrition is apt

to arise. Alcoholic stimulants also should be given in convalescence with considerable moderation; in most cases they are not needed at all, the proper nourishing food alone will be sufficient to restore the strength of the patient.

The above remarks may be regarded as mere outlines of a rational treatment of yellow fever, to be filled with the details by the good judgment of the practicing physician. A number of special medicines have been, besides, used in this disease by many physicians; but as they have not only failed to exert any influence upon the neutralization or elimination of the infectious poison, but perhaps rather done harm, I forbear to mention and discuss them. Nevertheless, I would by no means discourage the trial of certain remedies in this disease—empirical as the experiment may be—if administered upon some rational principle. For, if a specific remedy neutralizing the poison should ever be discovered, it will probably be by empirical experiment. Such experiments can only be advantageously made in a hospital, and by experienced physicians, accustomed to systematic scientific investigations; therefore, the general practitioner will do better by following the simple rules of treatment given above until some special and better method has been ascertained.

REMARKS ON THE PREVENTION OF YELLOW FEVER.

Although the subject of the prevention of yellow fever has always received the attention of the medical profession, it has never agitated the public mind to such an extent as since the occurrence of the epidemic of 1878. Aside from the numerous articles which have been written on this subject, so much has been said in speeches and lectures delivered, that I cannot properly conclude this treatise without likewise offering some brief remarks on the prevention of this disease.

Though, from all that of late has been said and written on this subject, it might be presumed that the prevention of a contagious disease was a most difficult and complicated task, it will appear more simple when examined closer, and in a systematic manner and form. Neither is there any novelty in the attempt made for

the purpose of preventing disease, for this subject has been studied for many years by the scientific men of all civilized nations, most particularly during the last fifteen years, since the occurrence of the cholera epidemic in Europe; and the treatises and articles containing the results of the experiments and investigations relating to it are quite numerous, forming a considerable part of the medical literature of these years. Besides, many of these experiments have been made in so thorough a manner, that, without addition, they may equally serve as a safe basis in devising means for the prevention of yellow fever.

As a contagious disease can only be prevented by the destruction of its specific cause, it is obvious that the *true nature* of the latter must *first be ascertained*, before any reliable means could be devised to arrest its activity. Without this knowledge the means employed will be *empirical* in their application, and in most instances, *as experience has sufficiently shown*, be attended with a *failure* of the object in view. Thus, if the disease, as some suppose, is generated by the gases arising from decomposing animal and vegetable matter, the destructive agent, or so-called "disinfectant," should possess the property of neutralizing these gases; or, if, according to another theory, it is produced by the presence of minute organisms contained in the air we breathe, the disinfectant should be destructive to the lives of these beings, and, accordingly, be brought into close contact with them; or, again, if depending merely upon certain climatic or meteorological conditions, as some people suppose, there can be no hope for relief, unless the climate could be changed; and, lastly, if propagated by morbid secretions derived from the diseased organism itself, the disinfectant should be capable of decomposing the poison.

Supposing the theory which teaches the origin of yellow fever from gases arising from decomposing animal and vegetable matter in the streets and houses of a city to be true, then the means of prevention would be quite simple, consisting in keeping the streets and the interior of houses clean from this matter, with fair prospects of being delivered from the yellow pestilence. But though clean streets and houses are very recommendable, and indicate a certain degree of civilization, this theory, as far as it

concerns yellow fever, rests, for the reasons already given, upon a very weak foundation.

Closely allied to this theory is the "bilge-water" theory, according to which the cause of the disease is to be sought either in the gases arising from this liquid, or in the minute organisms which it contains. If this theory was as true as it is false, there would be no difficulty of keeping yellow fever from our shores, as there is no want of agents which would neutralize the gases, or kill the organism, in a circumscribed space like the hold of a ship, though the proposed freezing process, having no rational foundation, would, like other empirical experiments, prove a decided failure.

Greater difficulties, however, would be encountered in the prevention of yellow fever, if the disease depended upon the presence of minute organisms contained in the air and breathed by the people, as here, the only effectual mode of destroying the cause would consist in bringing the organisms into actual contact with some disinfectant agent, as, for instance, carbolic acid *in solution*. Accordingly, every little corner of the streets and houses, as well as the furniture, bedding and clothes of the people should be wetted with this solution, a task impossible to be accomplished. To use the disinfectant in a gaseous form would be equally as impracticable, for the reason that, in order to destroy the organisms, it would be necessary to use the gas in such a concentrated form as to cause the death of the surrounding people, before it would in the least affect the organisms. For ten successive years the streets of New Orleans have been sprinkled over with carbolic acid solution, without ever having made the slightest impression upon the cause of yellow fever. It must, therefore, have now become obvious that unless these or similar measures are carried out in a most thorough manner, it is useless to resort to them at all.

If yellow fever were really caused by noxious gases arising from animal and vegetable decomposition in the streets and houses, or by the bilge-water in the ships, or by bacteria or other minute organisms, the prospects of mitigating or preventing the disease would not be very encouraging, as dirt and filth will occur in any large city, and in defiance of the best regulated and executed sanitary laws. But, fortunately, this is not the case, as it is

more probable that its true cause is a specific animal poison, similar in nature to that of small-pox, scarlet fever, or other contagious diseases, or to other animal and vegetable poisons,—substances, the noxious influence of which may, with the proper caution and attention, be counteracted, or, at least, to a certain degree limited.

In the preceding pages I have already sufficiently demonstrated the nature of the poison, and the manner in which it is propagated and carried from place to place, but have omitted to offer any remarks on the proper mode of prevention. In entering upon this subject it becomes necessary, first, to inquire in what manner the poison is probably introduced, or called into activity in a community. Thus, if yellow fever should appear at New Orleans, or any other seaport, *after having been entirely absent for several years*, it is very probable, or *almost certain*, that its poison has been imported anew, and most likely by ships hailing from infected ports. But, if the disease should re-appear in a place one or two years, or even longer, after having prevailed there, the probability arises that the poison is derived from the previous cases, having remained in the interval of time in a dormant (condensed) state, *i. e.*, for the want of the conditions necessary to its re-assuming the vaporous or gaseous form, as already explained. This probability, however, does not preclude a simultaneous fresh importation. While thus the poison may, on the one hand, be newly imported by ships, it may also lurk about from year to year in the same place, as is the case with small-pox, scarlet fever, measles, etc., the communicability of which nobody doubts. The extent to which the disease may spread, after having originated in this manner, cannot be determined, as it depends on various circumstances or so-called “chances.” It is more than probable that those sporadic cases, occurring almost every year, and whose origin cannot be traced, arise from these sources; and it is certain that the yellow fever epidemic of 1879, in Memphis, originated from the poison which, by adhering to the old clothes, had over-wintered in the shoemaker’s box. It will be remembered that in this epidemic the first cases occurred in one and the same neighborhood, and were confined to certain houses or families.

From the numerous experiments made relating to the nature of animal poisons or contagia and their disinfectants, almost every investigator has drawn the conclusion that the contagion will lose its noxious properties by a free exposure to the atmosphere, and that for this reason the latter, promoting the process of oxydation, is the best disinfectant; while, on the other hand, the poison will retain its properties when kept in a place from which the atmosphere and light are excluded. Now, it is well known that after an epidemic of yellow fever, the majority of people in whose houses cases and deaths have occurred, *do not destroy or even properly disinfect* the bedding or other articles to which the poison emanating from the patients has adhered: but that, on the contrary, especially among the lower walks of life, these articles are left as they were, and, in many instances, kept in dark and badly ventilated places, where there is no opportunity for the destruction of the poison by oxydation. As soon as the heat of the summer returns, and is accompanied by a certain degree of moisture, that portion of the poison, as yet not destroyed by oxydation or dry heat, will commence to be vaporized, and, if breathed by any individual susceptible to the disease, will manifest its activity in the production of the phenomena characterizing yellow fever. The first case thus produced may recover without its true nature ever being suspected, while the disease may nevertheless be communicated to other persons, and thus have existed for some time until a severe case, characterized by unmistakable and prominent symptoms, such as black vomit, occur. Under such circumstances the tracing back of the disease to the first case generally proves a failure.

These facts will show that in the prevention of yellow fever our attention must be directed toward two chief objects, viz., the prevention of its fresh importation, and of its spreading after once having made its appearance, and, furthermore, the sure destruction of the poison generated by the cases arising.

The first object, of course, can only be attained by a properly arranged and conscientiously executed *quarantine*. So much has been said and written of late, both officially and publicly, about this subject—especially by those who claim its study as a specialty—as to require no additional remarks. Nevertheless, it may

be said that the subject of quarantine has been presented as being much more complicated than it really is. Quarantine is quite an old institution, and has been practiced by many nations for many years. And it is more than certain that properly regulated and executed systems of quarantine do exist to-day, and, from all I have learned, even in our own country, which, moreover, might well serve as pattern for those stations along the coast of the Mexican Gulf, established for the purpose of preventing the importation of yellow fever. But though the administrative part of a quarantine may be quite simple, *as long as performed by intelligent and faithful officers*, the selection and application of a suitable disinfecting agent is more difficult; for, if this fails to destroy the yellow fever poison, when really present, the whole system of quarantine must appear a needless farce. The experiments made for the purpose of ascertaining the efficiency of a proper disinfectant must be made on matter, similar in nature to that representing the poison of contagious disease, and, as I have once hinted before in a short paper "On the Pathology of Yellow Fever"—published in the *New York Medical Journal*, Febr., 1879,—the fresh or dried lymph of the cow-pock-vesicle seems to be the most suitable for this investigation. A number of experiments of this kind have already been made by *Dougall, Baxter, Braidwood* and *Vacher*, all accompanied by nearly the same results. The summary of the results which Baxter obtained,—who extended his experiments even to the virus of infectious inflammation of guinea-pigs, and to glanders—is stated in his report as follows: *

1. "Evidence has been adduced to show that carbolic acid, sulphur dioxide, potassic permanganate, and chlorine, are all of them endowed with true disinfecting properties, though in very various degrees."

2. "It is essential to bear in mind that antiseptic is not synonymous with disinfective power, though, as regards the four agents enumerated above, the one is, in a certain limited sense, commensurate with the other."

3. "The effectual disinfectant operation of chlorine and potassic permanganate appears to depend far more on the nature of the medium through which the particles of infective matter are

*Reports of the Medical Officer of the Privy Council and Local Government Board.—London, 1875.

distributed, than on the specific character of the particles themselves."

4. "When either of these agents is used to disinfect a virulent liquid containing much organic matter, or any compounds capable of uniting with chlorine, or of decomposing the permanganate, there is no security for the effectual fulfilment of disinfection short of the presence of free chlorine or undecomposed permanganate in the liquid after all chemical action has had time to subside."

5. "A virulent liquid cannot be regarded as certainly and completely disinfected by sulphur dioxide unless it has been rendered permanently and strongly acid. The greater solubility of this agent renders it preferable, *cæteris paribus*, to chlorine and carbolic acid, for the disinfection of liquid media."

6. "No virulent liquid can be considered disinfected by carbolic acid unless it contain at least two per cent. by weight of the pure acid."

7. "When disinfectants are mixed with a liquid, it is important to be sure that they are thoroughly incorporated with it; that no solid matters capable of shielding contagium from immediate contact with its destroyer be overlooked."

8. "Aërial disinfection, as commonly practiced in the sick-room, is either useless or positively objectionable, owing to the false sense of security it is calculated to produce. To make the air of a room smell strongly of carbolic acid by scattering carbolic powder about the floor, or of chlorine, by placing a tray of chloride of lime in a corner, is, so far as the destruction of specific contagia is concerned, an utterly futile proceeding."

9. "When aërial disinfection is resorted to, the probability that the virulent particles are shielded by an envelope of dried albuminous matter, should always be held before the mind. Chlorine and sulphur dioxide are, both of them, suitable agents for the purpose; the latter seems decidedly to be the more effectual of the two. The use of carbolic vapor should be abandoned, owing to the relative feebleness and uncertainty of its action. Whether chlorine or sulphur dioxide be chosen, it is desirable that the space to be disinfected should be kept saturated with the gas for a certain time, not less than an hour; and this in the absence

of such gaseous compounds as might combine with or decompose the disinfectant, and so far impair its energy."

10. "When the thorough disinfection of a mass of solid or liquid matter, through which a contagium is disseminated, is impracticable, we should guard against giving a false security by the inadequate employment of artificial means. It is probable that all contagia disappear sooner or later under the influence of air and moisture, and that the absence of these influences may act as a preservative. When, therefore, we cannot advantageously or effectually supersede the natural process of decay, we must be sure that we do not hamper it by the injudicious use of antiseptics."

11. "Dry heat, when it can be applied, is probably the most efficient of all disinfectants. But, in the first place, we must be sure that the desired temperature is actually reached by every particle of matter included in the heated space; secondly, length of exposure and degree of heat should be regarded as mutually compensatory factors, within certain limits."

The above rules, laid down by Baxter for liquid or aërial disinfection, are very simple and will suffice for all purposes. In applying them, however, to the disinfection of ships, it will be found that a perfect disinfection of the whole cargo of a ship is almost impossible as long as the latter remains undisturbed, for the reason that the gaseous disinfectant cannot penetrate to its depth, or into the interior of the bales of merchandise, unless the ballast is broken. In order to thoroughly disinfect the latter, therefore, it must be broken, by unloading the ship and exposing it to the free atmosphere. Now, whether such a proceeding would be practicable, I am unable to decide, not being sufficiently acquainted with the commercial details and difficulties attending such an undertaking, I can only point to the fact, that unless every part of the cargo is exposed to the influence of the disinfectant or the atmosphere, any other mode of disinfection will be useless.

But, even, if the breaking of the ballast were practicable, a proper inquiry should first be made whether such a complicated labor is really necessary, that is, whether the noxious poison of yellow fever were, indeed, adhering to the merchandise of the cargo. Although I would not venture to deny the possibility of the poison being carried by the cargo of a ship hailing from the

sea-ports of the West Indian Islands, or of the coast of South America, and consisting, as far as I know, generally of coffee, sugar, tobacco, etc.—articles which are generally kept in large warehouses—I cannot but think that such an occurrence must be very rare, unless these warehouses are situated in a densely populated locality where the yellow fever is raging, and the goods are handled by persons who have come into direct contact with yellow fever patients. And even, if this were the case, it appears that during or before the act of loading, the goods, generally lying along the wharves of the port, become sufficiently exposed to the atmosphere to render the vaporization and ultimate diffusion of the poison into the latter possible. It is, therefore, more probable that the poison is generally carried to a ship by the clothes of the sailors and passengers, as well as by the effects locked up in their trunks; and it is from these that the poison is communicated to the usually badly ventilated and confined spaces and cabins of the ship during the voyage. This supposition will appear more reasonable in considering the nature of those places and localities in a sea-port, generally visited by the sailors when on land.

If the view I have taken is correct, a thorough disinfection of the clothes and effects of the sailors and passengers, together with the cabins and general hold of the ship, would be all that is needed.

The disinfection of the ship may be effected in accordance with the general rules laid down by Baxter, together with the washing of the deck and other wood work. But as regards the clothes and effects of the passengers, the application of dry heat is preferable, as it is more reliable in its effects. This method of disinfection has been practiced for a number of years in European hospitals, both in England and Germany, and different apparatuses have been constructed for the purpose, the description of which may be found in the medical records. With the construction of the proper buildings and accommodations of a quarantine station, arrangements could be made that, while the clothes and other effects of the passengers are exposed to the dry heat of the disinfecting apparatus, they, themselves, might enjoy a good bath, for soap and water will prove, after all, the best disinfectant.

The degree of heat which woolen or other goods are able to bear without damage in the heated chamber during the process

of disinfection has been determined by *Vallin*.^{*} He found that white blankets, if exposed to a temperature of 110° C. for two hours, slightly change in color, such as if they had been washed with hot water; if exposed to 115-120° for two hours, a slight yellowish coloration was noticed. Linen or cotton goods bear 115° very well, their color suffering only when exposed to 125° for two hours. The integrity of the goods becomes affected with a much higher (150°) temperature. Change in color occurs sooner with dry heat than when the heated air contains a large quantity of moisture. As regards the degree of heat required to destroy infectious matter, it appears that most germs of disease become innoxious with a temperature of 100° C., the septic matters, however, retain their power of infection even at this temperature.

The prevention of the yellow fever, after having been once introduced, or newly arisen—*events, possible in the face of the best sanitary regulations*,—appears to me no less important, and much more difficult, than to guard against its importation. Though it may be nearly impossible to extinguish the disease abruptly after it has once arisen, much can be done to prevent its spreading to a much greater extent, or from finally assuming the form of an epidemic. The only way by which this can be accomplished is to prevent, or, at least, restrain the intercourse between the healthy and the sick. I have already pointed to the most pernicious custom of the female neighbors entering indiscriminately the house in which a patient lies sick of yellow fever, either to inquire after his welfare, or for idle curiosity. Now, though the first of these objects is very laudable, it must be dispensed with, when associated with the danger of carrying the poison to other houses and families, from which it may eventually spread over the same or other districts. Any one, who will reflect upon the complexity of the daily intercourse between the inhabitants of a large city, will become convinced of this fact. The first step, necessary to the prevention of the spreading of the disease, must therefore consist *in informing the people of the contagious nature of the disease, and showing them the danger of entering a house infected with the disease*. *There remains no doubt but that extensive epidemics have chiefly been due to the idea of the non-conta-*

^{*} *Virchow u. Hirsh, Jahresbericht f. d. Jahr 1877. vol. I. p. 507.*

giousness of yellow fever, prevailing among the people. In advocating the strictest observance of non-intercourse with cases of yellow fever, I, of course, exclude those persons necessary to attend to the comfort and nursing of the patient. It is not my part to point out in this place the details of the manner in which this proposition may be carried out without too much inconvenience to the friends of the patient, but I am confident that with the proper system and precaution it can be done, and the disease be arrested, or kept in safe check until the colder weather sets in, when the fall of the temperature of the air will prevent the evaporation and diffusion of the poison.

Equally as important as the isolation of the cases is the complete disinfection or destruction of all objects to which the exhalations of the patient may have adhered during the course of the disease. Those articles with which the patient has come in immediate contact, such as bedding, shirts and other garments, should absolutely be burned, as soon as the patient is in convalescence, even if it should be done at the cost of the community, for it would be the cheapest mode of proceeding in the end. Such articles as will stand boiling and washing with soap should undergo the process, for, it may safely be guaranteed that the noxious poison, arriving in the gutters of the street after this procedure, will be surely deprived of its noxious properties; woollen articles may be disinfected by heat. As the poison adhering to the floors, doors and windows of the house is most effectually removed by plenty of soap and water, nothing would remain but the walls, which, if admissible, might be white-washed, or, if papered, thoroughly disinfected by one of the disinfectants in gaseous form, as recommended by Baxter.

From the above sketch it may be inferred that the prevention, or, at least, the checking of yellow fever, is not as difficult a task as might be supposed, if the proper precaution is observed, and the necessary means are conscientiously and systematically employed. A most important factor in the execution of these measures, however, will ever be the active co-operation of the inhabitants of the city themselves; the submission to some inconvenience by the individual for the welfare of the whole community, even at the sacrifice of a few individual rights.

Society Reports.

ARTICLE IX.

AMERICAN DERMATOLOGICAL ASSOCIATION: FIFTH ANNUAL MEETING. Held at Newport, R. I., August 30 and 31, and Sept. 1, 1881. Official Report of the Proceedings by the Secretary, Dr. Arthur VAN HARLINGEN, of PHILADELPHIA.

FIRST DAY.—MORNING SESSION.

ADDRESS OF THE PRESIDENT, DR. JAMES NEVINS HYDE.

Dermatology and its Relations to Periodical Literature.

GENTLEMEN OF THE ASSOCIATION:

In selecting as a theme for the annual address, one which covers the broad field described in the words, Periodical Dermatological Literature, I feel that at the outset I should recognize and impose upon it a necessary limitation. The two successive addresses of the distinguished gentleman who preceded me in this place, have furnished us with a valuable history of the rise and foundation of American Dermatology, including, under that name, a sketch of the current and completed periodicals published in this country in the interest of this department of medical science. Of those two addresses it may be said, that each is the fitting supplement of the other, and their publication, I might add, has attracted, beyond the borders of membership in this association, an interest which cannot fail to have an important bearing upon future contributions to the same general field.

It is my purpose, at this time, to briefly inquire, what have we a right to expect of Periodical Dermatological Literature in its attempts to supply the needs of the profession at large, with respect to a diffusion of knowledge as to the methods of establishing an accurate diagnosis in cutaneous diseases and as to the

means of their successful treatment. It is true that these questions are those whose answers must be familiar to all of you, and with respect to which I could not claim to be able to speak with any greater authority than yourselves. Still, it is on occasions like these when he who speaks in your name speaks with an added emphasis and a weightier than his own authority, that our utterances are received with the greater attention by the general profession of which we are all members.

Periodical Dermatological Literature exists in two tolerably distinct classes of publications. On the one hand, we find a list of special journals devoted chiefly to the consideration of diseases of the skin and kindred topics; and, on the other, the great body of current medical issues, conducted with a view to the advancement of general medicine or to its special departments distinct from that in which we find a special interest. The list of the former is limited and relatively small, not more than five or six of such journals now appearing in all countries, and of these none in Great Britain. The number of regular subscribers to these special publications is also necessarily limited; and it might be safely concluded that their pages are consulted by a still smaller number of general practitioners from the beginning to the end of the year.

When we turn, however, to the vast number of journals of general medicine published here and abroad, we find in them the field from which most medical men derive their knowledge, limited or the reverse, upon dermatological subjects. It is to the value of the articles which appear in their pages that the profession is indebted for its brightest successes in dermatology; it is to the deficient and erroneous teachings of the same sheets that we have to look for much of the ignorance and many of the vicious practices with which the consultant has reason to become familiar. I desire to ask your attention to this state of affairs for a few moments. And let me premise what I have to say with the remark, that large as has been the value of the contributions made by the members of this association since the day of its organization to special journals of dermatology, it may well be concluded that the work the same gentlemen have accomplished, in extending a knowledge of diseases of the skin by their communications to general medical literature, has been of equal value to the profession at large. I do not, however,

fail to appreciate the fact, that the general diffusion of knowledge regarding any special department of science is apt to be proportioned to the fruits of the labor expended in its particular channels of research.

Contributions of the class we have now in view, those, namely, which from their wider circulation secure a more extensive reading, may properly be assigned to four categories. There are, first, the solid and praiseworthy observations of fact, or deductions of a trustworthy character from the observations of others, respecting the etiology, diagnosis, pathology and treatment of diseases of the skin. There are, second, reports of improperly, because imperfectly, observed facts and crude opinions derived from these imperfect observations. One reads these with a feeling of regret that the author had not brought to bear upon his theme a riper experience or a more extended collaboration of the results of investigation made by others. Next, we have the worthless papers of men totally ignorant of the meaning of the terms and facts which they have attempted to employ, and of the significance of the relation of these to each other, or to others beyond the scope of their recognition. Unfortunately, the syntax and logic of these three varieties of literature are not always proportioned to their intrinsic value. If they were, the educated man might, from his general knowledge alone, be able to distinguish between the wheat and the chaff. Sometimes such proportion does exist, often it does not; and a certain degree of education in dermatology is thus needed in order to detect the counterfeit of the truth, when the latter is presented in an attractive form and under the cover of a name of authority.

A fourth class of papers is negatively a source of mischief, worked, however, chiefly to the authors themselves. These are written by men who should know well the subjects which they attempt to discuss, if we judge them from the standpoint of position and experience; but either from an undue haste, originating in the pressure of many duties, or from a foolish desire to identify themselves speedily with the subject upon which they write, or even from other motives not of the highest character, they deluge the medical press with voluminous papers filled with verbose iterations of ideas made public before their day, often by others in much better form, which serve to encumber the world with a little more of the rubbish swept annually by a pitiless indifference into the limbo of the forgotten.

Very much, however, has been accomplished in educating the mass of professional men in America with respect to the interesting and fruitful topic which we are now considering. An examination of the medical periodicals published in this country during the last five years, and a comparison of these with those of an earlier date, will bear evidence to the fact that in their appreciation of the importance of cutaneous medicine, in the accuracy of diagnosis and in the discussion of questions connected with obscure cases, there has been an improvement which has been as creditable for the past as it is promising for the future.

Permit me, however, to suggest very briefly some of the particular topics upon which there is room for a more extended knowledge among the mass of the profession, and where such information will make the immense difference between the brilliant success or the dismal failure of the average practitioner of medicine.

Surely it rests upon us to do our part in excluding from the discussion of the important questions presented to the minds of our generation, the ghosts and spectres of those which have preceded, and which have scared so many of our kin into the devious thickets on either side of the path of safety.

In the matter of diagnosis, for example, how common it is to find the forms of papular eczema mistaken for prurigo, merely because there was not the discovery of a vesicle, that ancient *sine quâ non* of an eczematous disorder. Here, again, is a supposititious eczema, long and vainly treated because of the failure of the practitioner to distinguish the lesions of pediculi inhabiting the clothing of the patient. I was lately summoned by one of the shrewdest physicians of my acquaintance for consultation in a case supposed to be one of obstinate eczema of the forehead, doubtless so designated by my friend in consequence of the vesicular character of the disease, which proved to be an exceedingly severe form of zoster ophthalmicus, the scars of which the unfortunate patient will carry to his grave. Erythematous eczema of the face as it occurs in middle-aged adults is another variety of the same disease whose recognition becomes too often a stumbling-block to the medical man clever enough to discover and relieve every species of laceration of the cervix uteri. Patients thus affected frequently drift about among physicians, now treated for "erysipelas," and now for some

"humor of the blood," often attributing their malady to the most remote and impossible of causes. The case of an intelligent lady occurs to me in this connection, who, when consulting me for an affection of this character, referred all her troubles to the incursion of an insect several years before into the respiratory passages, and who would consent to no remedial measures which did not contemplate the removal of the unwelcome and now venerable visitor.

Turning to another of the common diseases of the skin, acne, one would be disposed to conclude, that the ease with which it can be usually recognized, its frequent exposure in public to the eye of the uneducated man, would serve as a basis for a very wide-spread familiarity with its symptoms and character. Such, however, is, as you all too well know, far from the case. Truly, if we were less ignorant of things spread daily before our eyes, we should have less difficulty in apprehending the hidden and abstruse. Possibly, it is exactly because acne occurs so frequently upon the face, that so many odd errors are committed against its personality. Thus we find a patient who has at one time been infected with lues, and who has perhaps exhibited a series of syphilodermata, with a few tertiary lesions of periosteum or bone, leading a life of dissolute habits for eight or ten years thereafter, and subsequently developing one of the forms of rosacea about the nose, forehead or cheeks. The arguments necessary to convince the average medical man, that this is not a symptom of syphilis and is absolutely unconnected with the infective process, are sometimes scarcely within our reach. Fact and reason are often equally powerless to dissuade these individuals from pursuing for years a specific treatment, which is as often disastrous to the personal appearance of the patient as to the reputation of the practitioner.

Then, again, there is the case of the man who has been exposed to venereal disease in his youth, without having been really infected, the acne of whose maturer years is persistently regarded by himself as the pitiless revenge which time inflicts for his vicious employment of it in the past. A striking example of this class of cases once came under my observation. A respectably appearing gentleman, fifty-five years of age, and affected with severe acne vulgaris lasting for fifteen years, came to the consultation accompanied by his wife, to whom he had

been married for a quarter of a century. Just before the appearance of the rebellious acne, she had discovered her husband in improper relations with a woman of questionable character, and for fifteen long years the wife had reproached the husband for a disease which she supposed had been communicated to him, and the evidences of which she thought she saw in his face. The proof of these assertions upon her lips, corroborated as they were by many physicians, he had never questioned. A most careful examination revealed the true state of affairs, and it occurred to me, that the triumph of the husband as he found the stigma of his disease removed, seemed even less than the disappointment of the wife at finding herself deprived of her accustomed weapon of attack. Lastly, there is the man or woman, who is sexually an hypochondriac, and to whom the existence of a facial acne is a source of terror, as significant in his or her eyes, and in eyes, too, that should be better instructed, of poisonous or vicious matter in the blood, produced by disease of the generative organs. In the futile effort to dissipate or antagonize these "impurities," medicaments are swallowed for months at a time, with results which are often such as to transform pustules into large abscesses of the sebaceous follicles, and to produce a consequent facial deformity, which, if we followed the fashion of our ancestors in giving long names to symptoms, we should denominate the "*facies sexualis medicamentosa*."

How singularly the popular belief as to the generation of poisons in the blood, accords with the creeds of mediæval theology respecting the vileness of man! In the purview of the latter, the living body was a corrupt and evil thing; self-neglect, a duty; self-mutilation, a virtue; and self-immolation in desert or cell, the crowning glory of a perfect life. If it be charged that this is the age of the deification of matter, we may well retort that that was the day of the apotheosis of dirt! To it, I believe, may be traced the prevalent doctrine of blood impurities, as the voodooism of the American black is found to have its source among the traditions of the negroes in the heart of Africa. What more pitiful than to listen to a healthy man, the father perhaps of a healthy child, his face flushed with the ruddy hue of the current that sustains his life, able to digest roast beef and to enjoy sunshine, showing merely a few pimples upon his face, and describing his body in terms that would be

appropriate in characterizing a pile of ordure! In cutaneous medicine, it may not be true that, as Huxley said, "the people perish for want of knowledge," but it is certain that, for want of it, they endure a great deal of wretchedness and misery.

Turning to the somewhat rarer varieties of skin disease, we find that the common errors committed in their recognition are often as ludicrous as they are mortifying. You doubtless recall the case mentioned by one of our number, at the New York meeting, who had been consulted respecting a supposed case of leprosy, which was in fact one of melanotic sarcoma. The late president of the association recently informed me that his opinion had been asked with reference to a patient who had been assured by his physician that he was the subject of leprosy, and who was found to be suffering merely from an attack of tinea versicolor. It is a common experience in the last-named affection to encounter patients who have been treated for years for the relief of hepatic or other visceral disorder, and yet who are restored to their normal condition in the course of a few days by appropriate treatment as soon as they fall into the hands of a competent physician.

Lupus, again, is a disease of the skin, whose name has been employed to cover a multitude of mistakes. The various forms of carcinoma are so little differentiated, in the understanding of many practitioners, and it may be said also of many surgical authors, not only from each other, but from other lesions of less grave portent, that they are frequently confounded under the designation lupus. The fact that lupus vulgaris is, at its outset, most commonly a disease of early life, is not generally appreciated beyond the dermatological world, and the result is that it has served as a convenient halting-ground for many who are called on to give an opinion respecting suspicious solitary ulcers of advanced years, the real nature of which is not even suspected. Thus we find it used very largely by the men who do not hesitate to align themselves with sectarians in medicine, and whose knowledge of half truths is apt to be proportioned to the minimum of their doses. It is none the less remarkable of lupus erythematosus, that it is not rarely confounded with squamous eczema, and when persistent, combatted by remedies supposed to be efficacious in the so-called diseases of the blood, such as sarsaparilla and stillingia, those innocent weapons of

warfare, which ought one day to be displayed in an international museum of medical antiquities, to be gaped upon, wide-mouthed, by coming generations, as does our own upon the relics in the Tower of London.

I refer him who is interested in errors of etiology to the admirable paper on that subject, written by the first president of this association, and now a part of its record. It should be read by every American student of dermatology. Perhaps it may not be inconsistent with the theme I have chosen, to touch very briefly, at this point, upon the demands, implied or expressed, upon the exponents of cutaneous medicine, with respect to the important relation they sustain to periodical literature.

It is well-nigh impossible to mistake the drift of the wisest popular and professional thought, in its influence upon the practice of our time. I believe that, erroneous as are many of the conclusions in the philosophy of Mr. Herbert Spencer, they have powerfully operated to inspire the modern methods of research with a keener appreciation of the real relations sustained by man to the external universe, and the influence of those relations upon each of his physiological and morbid states. Studied in its proper light, the altruism by which Mr. Spencer would explain man's moral sense, and even his affection for his offspring, becomes the key to many of his diseases and to the most rational methods of their control. Man is thus discovered, with the devils by which he was formerly possessed cast forth, and he stands the innocent or guilty victim of the folly, ignorance or vice of himself or another. His sufferings, no less than his passions and emotions, become links in a long chain of causes and effects. Absolution for his physical sins of omission and commission can not be purchased by swallowing a pill or anointing himself with a salve, as readily as the burdens of his conscience are purged by the pardons of a priest. The attractiveness which the study of hygiene possesses for the modern investigator lies in its revelation of the possibilities of preventive medicine, if you will permit the use of such a misnomer, and I do not know of any field which, in this regard, is so promising as dermatology.

It is beginning to dawn upon the minds of men that it is easier to hurt than to heal; cheaper to foresee than to suffer; wiser to be prevented than to be punished.

We have sadly perverted, in these latter days, the primitive sense of the old Latin word *curare*—to take care of, or to care for ourselves or others. We have made it mean that part which man plays in the healing of the diseases of his fellow-man, a most detestable and wretched rendering of the original, with which the Italians are not chargeable. They still use the word *cura* in the sense of the care or treatment of a malady, while the verb *guarare* is applied to the healing or complete relief which is obtained by the cure. We find the term occasionally creeping into our own tongue with its honest meaning, as in the so-called grape-cure, or grape treatment of one class of maladies, and in like expressions. The true physician is really called on to cure, not only the sick, but also the well; to care for both, in the highest sense of the term, by preventing all their preventable disorders, and, in those which are not preventable, by preventing all preventable complications.

As for our prevalent employment of the word cure, it is safer to look upon it with a pessimistic than an optimistic eye. It is not only certain that in this sense we cure nothing, it is even probable that no inflammatory disorder of severity is ever perfectly "cured," even by the elegant processes of nature. We are dying from the cradle to the grave. These so-called diseases of ours are in reality steps toward the final dissolution, arrested or not, but never retraced. Do you turn to me and say, "This is ground which a dermatologist should, of all men, hesitate to assume?" I answer that what can be shown to be the law in one department of knowledge, will probably fit well into all others. Does a cured pneumonitis leave the affected lung as distensible and pervious as before? How often will a cured pleuritis delicately glue together the apposed membranous surfaces in a union which only the hand of the anatomist will sever? The extremities of a fractured bone are united, it is true, but we can read the story of the accident in the skeleton for generations after. What blennorrhagia leaves the urethra of the male, virgin of all accidents? When we turn to the skin, and discover that the prick of the lancet which penetrates beneath the mucous layer, tearing asunder a few of the epithelia, leaves there the indelible trace of its entrance; when we observe that the most perfect coaptation of the lips of a wound, made by a clean incision of the skin proper, is followed by the

formation of a new growth which no trick of man can remove, save by the substitution of another more cumbrous in its place; and when we reflect that scars, even half a century old, may reopen under the influence of scurvy, we may well ask of the presumptuous physician whether he will undertake to do better than all this?

We may look upon scars as the happiest results of natural effort to relieve a serious disease, and our best is scarcely better than that. What nature accomplishes with grace, delicacy and leisure, we effect brutally, coarsely and with despatch by knife, *écraseur*, curette or caustic. Let us call it "a cure," if we choose. It is really an arrested necrotic process, and the influences which decide whether that arrest shall be permanent or transitory are practically beyond our control.

This wonderful and complex organism which we call the skin is a living and sentient veil, stretched between the inner man and the outer world, with a series of complex organisms on either hand. However much we may conscientiously differ respecting the relative influence of the one side, or the other, in the production of its diseases, we can not fail to recognize the important fact of its interposition between the two. Even in those disorders which are admitted to be largely of internal origin, we discover that their study can not rest with the recognition of a single etiological factor, but must be pushed further to the investigation of those accidents which operate to give form, outline and position to all significant lesions. Thus, for example, Parrot has recently called attention to the fact that in hereditary syphilis the frequent occurrence of mucous patches about the anus is due to the accumulation over that part, of the fæces on the napkin of the child, and the labial sores of the same little patients are known to originate in the action of the lips on the nipple of mother or nurse. The close observer will be constantly surprising these external influences in their operation upon the living body, in every variety of pathological condition; and it would be as unphilosophical to assign the bed-sore of typhoid fever to the emanations from a cess-pool, as to conclude that the patches of *tinea versicolor* on the chest of the tuberculous patient were the result of a diathesis.

In this fragmentary way, I have merely alluded to a few of the special topics to which this subject naturally directs our

attention. Will you permit me to formulate, with a view to their distinct expression in this place, a few of the conclusions which are thus suggested? Some of the common errors, then, which require refutation, are:

First. That because a disease exists and is visible on the surface on the body, it for that reason alone requires active internal or external treatment. Surely the diseases of the skin share the peculiarities of those of many other organs of the body. Some need no treatment. He is, indeed, well fitted for his responsible position who advises his patient that the disease of the latter disturbs the imagination more than the body; is possessed of no element of danger to himself or another; will, if undisturbed, proceed naturally to its own favorable issue, and call for no intervention on the part of either physician or patient. These cases occur to all of us in our daily experience. I had, lately, the fortune to watch the evolution of forty cases of rōtheln, medically untreated in one of the public charities of Chicago. In no one of these was any drug administered, and in none did any accident occur worthy of mention, beyond the usual features of the disease.

Second. That because a disorder of the skin exists, it can be always readily relieved by an expert. There are cutaneous maladies which prove inevitably fatal. There are others of such an inveterate character that the skill of the most practised is inadequate to procure relief. Though not fatal, the latter are none the less persistent and remediless. Is it not curious that there should even be occasion to remind the world that the wisdom and skill of man have not yet reached the acme of success in all those emergencies where a demand is made upon the resources of the healing art? Are there no forms of non-specific catarrh which resist the most conscientious efforts of the physician? Are there yet no deformities which the skill of the orthopædic surgeon fails to remedy? There are thousands of men to-day on the pension rolls of the United States Government for chronic diarrhœa, which has been uncontrolled since its victims first contracted the disease in the swamps of the Southern marshes. It may well be regarded as an unintended compliment to the skill of the dermatologist that he is ever and again consulted for the relief of a cutaneous disorder which has lasted for sixteen or twenty years, and which he is

asked to remedy in the course of as many days. "A chronic disease," said Fournier, "requires a chronic remedy;" but, in the view of the uninformed, this axiom is reversed. There are inveterate diseases of the skin, as of other organs of the body, and, brilliant though be the success with which some of these are managed by the competent man, a variable proportion will bid defiance to the most practised hand, guided by the best trained judgment, till our methods are improved and our resources largely increased.

Third. That after relief is secured by treatment, many diseases of the skin can be so managed that they will not recur. The implied demand is made, it seems to me, upon the dermatologist alone. The patient with a fractured leg does not ask of his surgeon a guarantee that he will not suffer a similar accident, if he again fall beneath the wheels of a rolling car. The convalescent from a follicular enteritis, does not demand a future immunity from errors of diet in an unfavorable season or unpropitious climate. And so the patient, with an intertrigo beneath her pendulous breast, or a dermatitis medicamentosa, should not expect to be protected from the results of either a neglect or an imprudence. Some diseases of the skin recur continually, and will do this for a lifetime. It is true that these recurrences usually result from adverse extrinsic influences, but it is often not in the power of the physician to set these aside. In such cases he discharges his full duty, when he forecasts the future and describes the result of the best treatment under forbidding circumstances.

Fourth. That the expert, upon the examination of but a portion of the affected integument, is competent to pronounce at once upon the character of a disease. It is true that in many cases a large experience enables such a man to recognize at a glance the distinctive features of a malady. In the same way the surgeon can occasionally, and with equal readiness, recognize a case of talipes, and the oculist one of convergent squint. But no opinion is worth the time of the patient or consultant, which is not based upon a most careful, exhaustive and patient investigation, not only of the visible symptoms present to the eye upon the surface, but also of the history of the case and all other pathological facts obtainable. It is, indeed, the skill with which these are discovered and assigned

their proper influence in a decision as to the nature of the complaint, that constitutes the chief difference between him who is, and him who is not, expert in diagnosis. Strange, indeed, that the dermatologist alone should be supposed absolved from this imperative and fruitful labor in the study of his cases, yet such an idea is sufficiently wide-spread, largely among the laity, and to a certain extent among the profession. It is a common experience to be shown a patch of diseased skin or a few distorted hairs, upon which follows the inevitable question, "What do you call this?" I was lately stopped on the street by a physician of large practice and wide reputation, doubtless on the way to the bed-side of his patient, who hurriedly demanded, "Please tell me how to treat eczema!"

Fifth. That a practicing physician need not always make an accurate diagnosis in cutaneous disease, and that a failure on his part to do this, need not be considered as perilous to himself. In a recent inaugural address, delivered before the class of one of the medical colleges in the city of New York, * the speaker remarked, "The physician must never forget that his patient is, from the nature of things, a possible enemy." Well and truly said. The time has nearly passed when our intelligent countrymen, seeking to know the nature of their complaints, can be put off with a long-sounding name and a prescription written in a dead language. For many of them it will no longer suffice to be informed, in the matter of diagnosis, that they are affected with "a disease of the skin." They ask, many of them, to know what form of skin disease is theirs, and, if not satisfactorily answered, it requires but little to effect this transformation of a friend into an enemy. The fact will finally dawn upon them that they have been in charge of one who really could not even name the disease which he had dared to treat. "What," they properly exclaim, "would be thought of a surgeon who would undertake to treat a fracture, not knowing whether it were a dislocation or a rheumatic affection of a joint, and what would be the position of a merchant who, in the commercial world, could not exactly name the amount of his liabilities?" Sooner or later, patients of this class find out for themselves the persons who are competent to describe their condition and to advise them judiciously with

* Inaugural Address. M. Putnam Jacobi, M.D., CHI. MED. JOUR. AND EXAM., June, 1881, p. 561.

respect to its management. We, who often are consulted at this juncture, are sometimes compelled to listen with a feeling of shame for our profession, to indignant reproaches on the lips of those who, but a brief time before, trusted to the honor and relied on the skill of our brothers in medicine. It is none the less pleasant to record the attested fact, that the man who is candid enough to admit that he is in doubt respecting the nature of the disease for which he is consulted, and who summons another, in order to avail himself of a wider experience or a readier skill, rarely fails by such a course to strengthen those bonds of sympathy which should always unite the physician and patient. In the vast majority of these cases, patients return to their physicians with an heightened sense of the trustworthiness of the latter.

Let no man interpret these statements in the light of a plea for a wider range of possibilities for the consultant. The educated gentlemen who have made themselves masters of any special department of science, in this country, certainly have found no lack of appreciation and that reward for skilled labor which all honest work inevitably yields. They surely need to have no plea entered for them. This association has steadily set its face in the direction of the needs of the physician in general practice with respect to a knowledge of cutaneous diseases, and has made a most important and valuable advance in the diffusion of such knowledge in America. If these words of mine can be interpreted as a plea, may they be held as pleading for higher attainments on the part of the general practitioner.

Sixth. That such remedies as mercury, arsenic and the iodide of potassium may be administered in any case where an accurate diagnosis has not been established, and in such cases may be used unadvisedly. Said the physician of a past generation, "When you do not know what else to do, treat for syphilis." It is difficult for me to touch upon this point, without the appearance of exaggeration. Doubtless the experience of many of you does not greatly differ from my own. I am quite prepared to say, that for one patient who is benefitted by the administration of these remedies in the hands of incompetent men, there are ten who are made worse. The vulgar idea that all disorders of the integument are, in some obscure way, to be connected with a syphilitic taint in one generation or another;

the erroneous opinion that syphilis descends through many generations and lurks obscurely in the eczema of the infant, the psoriasis of the adult, the sycosis of the young man, or the acne of the young woman, these are the spectres that scare the foolish, whether they call themselves doctors or laymen, into the rough régime of a crude *materia medica*. It should be considered a crime to subject a man or a woman to a systematic mercurial course for the relief of an affection supposed to be syphilitic, but in reality not such. The aggravations induced in acute cutaneous exudations by the early and indiscriminate employment of arsenic, need not be described to those who, like yourselves, have the opportunity of observing these conditions. Acne-sprinkled faces, which for months have exhibited almost every one of the various eruptions induced by the ingestion of the iodide of potassium, are the staple of our public clinics and the reproach of our body medical. Who will write, in figures that should astonish the student of medical statistics, the number of all diseases of the skin, readily manageable at their outset, but after long subjection to the so-called "driving-out" process, rendered either inveterate or remediless?

Seventh, and lastly. That the study of diseases of the skin is the pursuit of a narrow specialty, leading to a knowledge of superficial eruptions upon the surface of the body, and to an ignorance of the functions and maladies of important internal organs. The erroneous impression thus suggested has contributed to the too general disregard of cutaneous medicine, not only in the medical colleges and charities of our country, but also, in part as a consequence of this, in the studies of professional men. Yet he who would specially devote himself to the investigation of the disorders of the skin requires, fully as much as his professional associates, the amplest preparation in a finished general medical education and experience; and when at last ready for his work he will discover, if he has not before done so, that he needs to be a physician in the highest sense of the term, and the peer of all his fellows. There is no organ of the body, either in health or disease, which it may not become his duty to explore and investigate, for the skin is only one of the living and potential microcosms which constitute the macrocosm of the human frame. Surely I need not remind any of you, that he can ill afford to neglect the study of diseases of the skin, who

devotes himself especially to the maladies of the eye, of the ear, of the generative organs of either sex, of the nervous or other system of the living body. The same is true of the surgeon, the accoucheur, and of the professional man who is called to treat the disorders of infancy and childhood.

The added power and skill of him who acquires the knowledge of which I speak is something which its possessor alone can fully appreciate, and of him who has it not, I think you will agree with me in saying, that as a diagnostician he is fatally deficient. Who can describe to one that knows nothing of it, the sense of gratification afforded by the ability to read at will the page presented on the surface of the living body, its secrets utterly sealed to another, who finds these characters meaningless ! Who will declare the marvellous manner with which nature rewards such a man for his patient, minute and watchful attempts to translate these characters, giving them a clearer significance and more legible distinctness for every added hour of attention ! Who, that has once enjoyed this enlarging reward for his toil, would exchange it for the power to decipher any hieroglyphics that were ever graven by man upon a monolith, or imprinted by his pen upon a palimpsest ! His is a truer palmistry than that which seeks only to read the secrets of the inner man ; for to him the human hand, since first the pink of its unused palm was laid upon the breast of a mother—and whether it betray the syphilis a decade old, or the manner of toil by which man earns his bread—tells ever a true tale of its acquaintance with that part of the world which it was fashioned to grasp.

In the hope, Gentlemen of the Association, of making this address of some practical value, and also of further justifying its title, I have appended to it a general alphabetical index, by authors and subjects, of all the original communications which have appeared in the pages of our special dermatological journals, including also the papers written by members of the association and published elsewhere. The index, therefore, is made from the following periodicals :—

The *Vierteljahresschrift für Dermatologie und Syphilis*, Wien, 1875 to July, 1881. Current.

The *Archives of Dermatology*, New York, 1874 to July, 1881. Current.

The *Archiv für Dermatologie und Syphilis*, Wien, 1869-1873. Complete.

The *American Journal of Syphilography and Dermatology*, New York, 1870-1874. Complete.

The *Annales des Maladies de la Peau et de la Syphilis*, Paris, 1844-51. Complete.

The *Annales de Dermatologie et de Syphiligraphie*, Paris, 1869 to July, 1881. Current.

The *Archiv für Syphilis und Hautkrankheiten*, Berlin, 1846. Complete.

The *Giornale Italiano Delle Malattie Veneree E Delle Pelle*, from the 11th year to July, 1881 (the first ten volumes being omitted.)

The *Journal of Cutaneous Medicine and Diseases of the Skin*, London, 1868-1871.

The index, thus prepared, is, I am aware, too large to be incorporated with the proceedings of this association. I shall therefore hope at no distant day to place it in the hands of the members of the association in a printed form, without adding to the volume or the expense of our annual transactions.

The President's address having been delivered, DR. CHARLES HEITZMANN, of New York, read a paper entitled

A Contribution to the Minute Anatomy of the Skin,

of which the following is an abstract :

The apparently complicated structure of the skin is easily understood if it is kept in mind that there are but two main tissues entering into the structure, viz., connective tissue and epithelium. The former constitutes the derma, and is arranged below in a coarse felted network and above in fine delicate fibers without distinct arrangement. The superjacent epithelial layer is subdivided into two strata, the lower (*rete mucosum*) alive and supplied with nerves, the upper (epidermis) of dry, horny epithelia. The connective tissue is supplied with blood-vessels and lymphatics, the epithelium lacks such structures.

If, now, considering the formation of the hair, we imagine the connective tissue and epithelium together as a pliable sheet as of chamois, and if in this a depression is produced by pressure with the end of a finger, the result will be a pouch, the innermost layers of which are epithelial, while the outer layer is connective tissue. The epidermis covering the inner surface

of the pouch now bears the name "inner root sheath." Next to this will be a layer formed by the epithelia of the rete mucosum, the "outer root sheath." The outer portion of the pouch composed of connective tissue represents the "follicle." On the bottom of the pouch a protrusion upwards entirely analogous to the papillæ, and formed like those of connective tissue, constitutes the papilla of the hair.

In the formation of the pouch the horny epidermic layer is much thinned in its involution, becoming attenuated in the middle to two rows of cells, thickening again lower down to three or four strata of epithelia, which have lost their horny character and have again become protoplasmic. The rete mucosum enters the pouch in full width, but becomes narrower; the cells, however, retaining their protoplasmic character, until it is reduced to a single layer and finally disappears.

If we imagine the bottom of the pouch pushed upward, this prolongation, from the structure above described must consist of the inner-root sheath: it represents the hair, which is therefore a solid elongation of the hollow inner-root sheath; the outer root-sheath has nothing to do with the formation of the hair. At the bottom is the bulb of the hair, composed of living protoplasmic epithelia. Higher up in the hair the epithelium becomes horny again to form the shaft of the hair.

The writer went on to describe the formation of the sebaceous follicle from the outer root-sheath; the inner root-sheath has nothing to do with this formation. The pouch of the sebaceous gland is under the control of the arrector pili muscle, which assumes the form of a flat fan-like sheet, the broad ends of which terminate in the papillary layer, while the narrow end is inserted into the follicle. No doubt the evacuation of the sebum is effected by the contractions of this muscle, the fatty matter being squeezed into the funnel of the hair pouch and not directly out to the surface excepting in the largest glands.

After giving some further account of the minute structure of the hair and of the process by which it is shed and renewed, the writer went on to describe the formation of the sudoriparous glands. We may simply assume a prolongation of the outer epithelial layers into the deeper portion of the derma. In the epithelial layer of the skin, where the cork-screw-like windings are observed if this is thick, as on the soles, the sweat duct is

lined with flat epithelia. At the point where the duct pierces the rete mucosum, in the valley between two papillæ, it is lined by stratified epithelium. Within the derma, the duct is composed of a single layer of columnar epithelium, obviously a prolongation of the lowest layer of the rete mucosum. The tube, as it begins to coil up, is lined by a single layer of cuboidal or short columnar epithelium. Both the duct and the coil are invested by a somewhat denser connective tissue and supplied with blood vessels. Between the connective tissue and the epithelium there is sometimes a distinct structureless or hyaline layer. Larger coils, especially those of the axilla are supplied with smooth muscles, which are imbedded in the ensheathing connective tissue capsule.

DR. LOUIS A. DUHRING, of Philadelphia, congratulated Dr. Heitzmann on his paper, remarking that the facts there presented would greatly simplify the heretofore complicated views as to the relations of the various structures of the hair and its follicle.

DR. JAMES C. WHITE, of Boston, asked Dr. Heitzmann how it happened that he had never before observed the arrangement of the various layers as described in his paper.

DR. HEITZMANN said that he, like others, had formerly followed the recognized authorities. Recently he had gone to work independently. In answer to a question put by DR. EDWARD WIGGLESWORTH, of Boston, as to the formation of the hair pigment, Dr. Heitzmann further remarked that he had not as yet entered into the study of the subject.

DR. JAMES C. WHITE, of Boston, read a paper on

The Limitations of Internal Therapy in Skin Diseases.

The writer said he would like to approach the consideration of the subject from a purely empirical stand-point, uninfluenced by theories and traditions, and supported only by the results of observations made and recorded in ways recognized as strictly scientific. Cutaneous pathology offers an exceptional field for the study of drugs introduced through the stomach; for, in addition to the ordinary means of judging their effect, the phenomena of tissue-change or modification of function occur directly under the eye. Yet cutaneous internal therapeutics is no more fixed or satisfactory in its results than the therapeutics of general medicine. In spite of great activity and the fact

that novelties even called specifics in skin diseases are scattered all along the history of medicine, only a few agents have attained and preserve a well-established reputation as serviceable in limited fields of usefulness. Dermatologists of repute in different countries still adhere to certain remedies from motives of conservatism or patriotism, while those of other countries cling to their pet drugs and regard foreign treatment as obsolete and erroneous. This unsatisfactory and contradictory condition of affairs must continue until better methods of study are adopted and the faults of individualism eradicated.

It would not be boastful to say that nowhere does there exist a body of dermatologists so broadly cosmopolitan in spirit and training, and representing such varied opportunities of research as are afforded by wide differences in climate and race, or one more competent for such work, than the gentlemen who compose the American Dermatological Association.

The writer went on to sketch an imaginary committee who should conduct a systematic investigation of new drugs. When a member of the association should present a paper on the specific effect of some drug in this or that affection, this would be submitted to the committee, who should follow some well-established procedure in testing its merits. In this way the association would do much to discourage the loose writing and the claims based on insufficient observation which generally herald the introduction of each new remedy. To such a committee might be referred not only all appropriate communications presented to the association, but all doubtful questions of importance in therapeutics, which now vex us so frequently, of old or new origin.

The following subjects of investigation might be cited by way of illustration:

1. The action, beneficial or injurious, of our most celebrated springs in affections of the skin. A report on this subject would do much to clear away the atmosphere of charlatanism which has hitherto obscured it, and develop some knowledge of certain characters regarding it, which the profession of this country has so long needed.

2. The power of electricity over the so-called neuroses of the skin (including zoster).

3. To what extent may certain substances in common use

be applied to the skin without danger, viz., compounds of zinc, lead, mercury, carbolic acid, etc.?

It is unfortunate that so few beds in hospitals are under our control, and that we must rely upon the voluntary visits of out-patients. But, even with such opportunities only at our command, much may be done.

The speaker then went on to speak of the vague and meagre knowledge possessed regarding the action of drugs on diseases of the skin, as illustrated in systematic treatises, going over each class of diseases *seriatim*, and stating the amount of exact knowledge possessed as to the influence of drugs internally administered on each. The number of individual affections of the skin recognized by us approaches closely one hundred. How many of these are under the direct control of internal medication in any marked degree? The speaker wished he might claim such power over ten of them—such power as arsenic may exhibit in psoriasis, as mercury almost always does in syphilis, and which makes external treatment unnecessary.

The writer did not mean to deny that much may often be done to hasten their good work by the simultaneous use of remedies which elevate the general tone of the patient's functions and tissues, when needed, together with local treatment. The whole patient must always be considered. Moreover, the writer did not believe that the narrow limit of our exact knowledge on this subject could not be enlarged. He would encourage any experimental studies, empirical or philosophical, in this direction. It is the manner in which this important subject has been hitherto so generally treated, so detrimental to scientific progress, which he deprecated. But a frank confession of our present inabilities is an essential step in any real advance.

DR. HEITZMANN, of New York, expressed his pleasure at hearing such views expressed. Men like Erasmus Wilson had done serious injury to dermatology by hastily asserting the influence of internal medication in diseases of a purely local character and indiscriminately.

FIRST DAY.—EVENING SESSION.

DR. ARTHUR VAN HARLINGEN, of Philadelphia, read a paper on

A Case of Lymphangioma Tuberosum Cutis Multiplex, giving an account of a woman, 30 years of age, who displayed a large number of tumors of various sizes and shapes distributed here and there over the whole body, together with numerous teleangiectases and irregular brownish patches of pigmented skin. The tumors in some cases resembled flabby molluscum fibrosum growths, while in other cases they were smooth lilac or bluish elevations from pin-head to hazel-nut size, and so compressible under the finger as to feel like bladders filled with air and to give the sensation of an umbilical hernia in a child. On excision they were found to consist of a pearly gelatin-like semi-transparent mass, with a smooth, bulging surface and without fluid contents. Microscopic examination showed the structure to be composed of fibrous and granulation-cell tissue, with numerous irregular spaces,—sections of dilated lymphatic vessels. But two other cases are on record, one described by Kaposi in Hebra's work on Skin Diseases, the other reported by Pospelow in the *Vierteljahresschrift für Dermatologie und Syphilis*.

DR. HEITZMANN, of New York, said that the writer was mistaken in thinking that only two cases had been reported. Billroth had reported a case of large lymphangioma, and a student of his had collected and published a number of similar cases.

DR. VAN HARLINGEN replied that he had examined the article alluded to and considered the cases of a different character from that he had just described.

THE PRESIDENT, DR. HYDE, of Chicago, alluded to Dr. Busey's work on the subject of disorders of the lymphatics, remarking that in the cases there reported, an escape of lymph, sometimes to a debilitating degree, was reported to have taken place.

DR. I. E. ATKINSON of Baltimore, asked the writer how he distinguished the molluscoid tumors described from true molluscum fibrosum, and went on to speak of cases where the tumors of true molluscum fibrosum could be pushed down into the skin as in Dr. Van Harlingen's case.

DR. WIGGLESWORTH reminded the writer of a case of mollus-

cum fibrosum described by himself several years ago, in which several symptoms were observed of a similar character to those noted in the paper just read, notably the compressibility and the gelatinous appearance on section described by Dr. Van Harlingen.

DR. LOUIS A. DUHRING, of Philadelphia, read a paper on

The Small Pustular Scrofuloderm,

consisting of a report of three cases of the disease, together with a general description of the symptoms of the process, the probable cause, and the diagnosis. It may be briefly described as being characterized by pinhead and small split-pea sized yellowish pustules with a more or less raised violaceous fixed base. The lesions are disseminated, and usually appear on the extremities, especially the hands and forearms. In general appearance they resemble those of the small pustulæ syphiloderm. They pursue a slow course, and in the course of several weeks begin to crust over with a small hard or horny, grayish or yellowish, adherent crust, which in time drops off, leaving a marked, punched-out looking scar. The lesions appear at irregular periods, new ones coming out from time to time, the whole process lasting months or years. Symptoms of general scrofulosis are usually present, and hence the disease was supposed by the writer to be a cutaneous manifestation of scrofula. It is liable to be confounded with syphilis, with acne, and with follicular lupus. The follicles, however, here are not invaded primarily. The writer stated that the disease had been first described by him in the second edition of his Treatise on Diseases of the Skin, and that it presented such marked features and was of such importance as to be worthy of careful study.

DR. HEITZMANN, of New York, asked Dr. Duhring if he was familiar with the clinical appearances of the acne cachecticorum of Hebra. So far as he could judge from the account of the cases just given, there seemed a close resemblance between the lesions there described and those of acne cachecticorum.

DR. DUHRING replied that he was familiar with the acne cachecticorum of Hebra, both clinically in Hebra's wards and by study and examination of pictures of the disease. The eruption was quite different from that which he had just described. In

reply to a question by Dr. White, Dr. Duhring said that the follicles were not involved in his cases.

DR. J. C. WHITE, of Boston, said, speaking of the etiology of the affection, that he doubted if scrofula could influence the skin to such an extent.

DR. GRAHAM, of Toronto, mentioned a case similar to those reported by Dr. Duhring, which had come under his observation.

[CONCLUDED IN DECEMBER NUMBER.]

ARTICLE X.

ILLINOIS STATE BOARD OF HEALTH.

The regular quarterly meeting of the Illinois State Board of Health was held at the Grand Pacific Hotel, Chicago, Thursday and Friday, Sept. 29 and 30, 1881.

All the members were present, viz: T. M. Gregory, LL.D., President; Anson Clark, M.D.; W. A. Haskell, M.D.; R. Ludlam, M.D.; N. Bateman, LL.D.; John McLean, M.D.; John H. Rauch, M.D., Secretary.

The minutes of the previous meeting were read and approved.

Dr. Jas. M. Bryden, having been previously notified that complaints had been lodged against him for advertising improperly, and contrary to the rules of the Board, appeared in his own defense. He is a graduate of the College of Physicians and Surgeons, of Indiana; formerly practiced in the city of Chicago, but is now practicing in Fond du Lac, Wis., and has been advertising in the papers of that city, calling himself the *Scotch* physician, and "displaying" a wood cut of himself. He admitted that he left the State because he thought his certificate would be revoked if he continued his advertisements.

Moved, That on condition of withdrawal from the papers of all advertisements except the ordinary physician's card, and without the wood-cut of his portrait being inserted, the prosecution of this case of Dr. Bryden shall be dropped. Carried.

The following resolution, introduced by Dr. McLean, was adopted by the Board:

Resolved, That in all cases where information comes to the notice of the Secretary of the State Board of Health that persons holding certificates from said Board are advertising in an unprofessional manner, or in any way are guilty of unprofessional and dishonorable conduct, it shall be the duty of the Secretary to notify them at once to appear before the Board, and show cause why their certificates should not be revoked.

Dr. Ludlam offered the following notice to appear before the Illinois State Board of Health :

WHEREAS, Complaint has been made at this office that, as a licentiate of this Board, you have been guilty of irregular and unprofessional conduct ; and whereas, if such charges should prove to be true, it will be necessary, under the statute, for the Board to withdraw your certificate, therefore you will please be notified herewith that an opportunity will be afforded you at the next meeting of the Board to present such papers and evidence in explanation and defense as you may have to offer in said case.

The case of Julius L. Lins, of Wallingford, Will county, was then considered. Lins made an affidavit in 1878 that he was a graduate of the University of Marburg, Germany, and had practiced medicine in Illinois ten years prior to July 1, 1877. Because of his ten-years practice he was not compelled to present his diploma for verification, *at that time*. The truth of this affidavit being questioned, the Dean of the Faculty of the University of Marburg was addressed, and the fact that he had never graduated there was ascertained. The Board ordered that his certificate be revoked and cancelled.

President Gregory laid before the Board a letter from Dr. G. H. Leach, of Cairo. The letter complained that W. H. Marean, of Cairo, had a certificate of practice from the Board to which he was not entitled. Intimations were also made as to improper methods of issuing certificates by the Board.

Dr. Rauch offered the following, which was adopted :

WHEREAS, Dr. G. N. Leach, of Cairo, in a communication to the Attorney-General of this State, dated Aug. 23, 1881, made

certain charges in connection with the case of Dr. Marean, of Cairo ; therefore, be it

Ordered, that Drs. Leach and Marean be cited to appear before this Board at its next meeting, to show cause why the certificates issued to each of them should not be revoked.

Dr. Rauch read his quarterly report as Secretary, of which the following is a brief synopsis :

Since the last meeting 553 letters and 421 postal cards have been written ; 5,552 circulars and 1,850 packages have been distributed, including 1,820 reports of the Board and Register of Physicians and Midwives. Certificates have been issued to 92 physicians on diplomas, 5 on years' practice, and 3 upon examination ; 8 to midwives on diplomas or certificates of examination, (nearly all from foreign institutions), 3 on years of practice, and 5 after examination. One diploma has been held for further consideration, one was withdrawn while being considered, and 23 doctors were compelled to leave the State because of their inability to comply with the law. An inquiry has been received from Portland, Oregon, where a diploma from the Edinburgh University, of Chicago, was presented to the Medical Director of the A. O. U. W. He was at once informed of the fraudulent character of that institution, and the action of the Board with regard to the same. The distribution of the official register makes the work of the Board less onerous, although there is still considerable annoyance from itinerant quacks who claim the benefit of the ten-year exemption clause.

In view of this fact, I would respectfully recommend that the Board take the necessary action, defining that section of the statute (*vide infra*).

Small-pox has appeared at five new points, and it will continue to occur while immigration is so heavy from Europe. Because of this probability, the Board should urge the protection of all by vaccination and re-vaccination, and I would also urge the Board to pass a rule making it obligatory upon the authorities of any township, town or city, to report promptly to this Board the outbreak of any contagious or epidemic disease, as I believe it would be of the greatest benefit to all parties concerned. The experience of the last nine months, especially in connection with small-

pox, has impressed me with the necessity of the preparation by the Board of a sanitary code of ordinances, for the adoption by local authorities throughout the State (*vide infra*).

Another important matter to which my attention has been frequently called during the last three months, is the condition of the Illinois River. In view of the sanitary importance of improving its polluted condition, it is clearly the duty of the Board to do all it can to hasten the construction of the pumping works at Bridgeport.

RAILROAD SANITATION.

Although a subject of the utmost importance, and one affecting a large majority of our population, it has not yet received the attention which it demands. The hygiene of the emigrant trains is notoriously bad, but even the more elegant means of travel are far from faultless. During the past year travel on all lines has been exceedingly heavy, and this has tended largely to aggravate the already existing evils.

As bearing directly on this subject, I call the attention of the Board to the following correspondence :

OFFICE OF THE SECRETARY,
SANITARY COUNCIL OF MISSISSIPPI VALLEY,
CHICAGO, May 24, 1879.

To COL. D. N. WELCH, *General Superintendent Pullman Palace Car Co. :*

SIR : Enclosed herewith I transmit, for your consideration, a slip containing the substance of a discussion on railway sanitation, before the National Board of Health, on May 7, at Atlanta, Georgia, as also the propositions adopted by the Sanitary Council of Mississippi Valley.

Will you please inform me what measures are now adopted by your company to keep the sleeping-cars in good sanitary condition ; and also what you did last summer and autumn, during the prevalence of yellow fever, and what disposition was made of the cars in which yellow fever patients were carried ?

In this connection I beg to offer certain suggestions, upon which I should be pleased to have an expression of your judgment. In my opinion, the upholstering of parlor and sleeping-cars is too heavy, either for comfort or health, in summer. Could they not be put in what might be called a *summer dress* from May to September, the seats be furnished with cane bottoms and backs, or covered with leather or enamelled cloth ? or, if the plush upholstery must be retained, then cover it with linen or other material that could be often changed and washed ; substitute the same material for the heavy woolen curtains, and dispense with carpets entirely ? Woolen stuffs absorb and retain waste matters much more readily than linen, and are much more difficult to cleanse ; consequently they are much more liable to convey infection. From a strictly sanitary standpoint, it would probably be better to construct a different style of sleeping-car, for use exclusively south of latitude 37° N. A car starting from Chicago and running through to New

Orleans is frequently subjected to great extremes of temperature. Unless provision can be made both for summer and winter ventilation, such extremes must necessarily be deleterious to health, aside from the question of comfort.

I am also of opinion, in common with other sanitarians, that long distance through-cars—as, for example, from this city to New Orleans, or to New York, or from New Orleans to Washington, are not conducive to public health, but, on the contrary, are often promoters of contagious and infectious diseases. The change to a fresh and clean car. at Cairo, Pittsburgh, Buffalo, or Atlanta, as the case might be, would no doubt diminish the risk of contagion or infection, if, indeed, it did not add to the safety of the traveling public in other ways; and such a change could be made with little inconvenience or trouble to passengers.

Very respectfully,

JOHN H. RAUCH, *Secretary.*

PULLMAN PALACE CAR COMPANY,
GENERAL SUPERINTENDENT'S OFFICE,
CHICAGO, June 16, 1879.

JOHN H. RAUCH, M.D., *Secretary Sanitary Council of Mississippi Valley, Chicago.*

DEAR SIR: In reply to your letter of the 24th ult., asking for information as to what measures are now adopted to keep sleeping cars in a good sanitary condition; also what we did last summer and autumn, during the prevalence of yellow fever, etc., I have to say that, immediately on receipt of your letter, I forwarded it to the General Superintendent of our Pullman Southern Car Company. After receiving his full report on the subject, I forwarded the papers to the Superintendent Central Division of the Pullman Palace Car Company, whose reports, and also that of Major Hays, General Superintendent P. S. Car Company, will be found herewith.

You will perceive, by examination of the two reports, that the questions you ask are pretty thoroughly answered therein; therefore it is not necessary for me to go particularly into details. Ever since the organization of the Pullman Palace Car Company, up to the present time, the question of the free use of disinfectants and detergents has been one to which the company has given much attention. The formula for the disinfectant now in use is the one recommended by the American Public Health Association, at a session held in New York in 1872 or 1873, and since its adoption it has been constantly in use in our cars; and we are informed by medical men and chemists that there is no better disinfectant for the purposes for which it is required. We had passed through two seasons of yellow fever on our coast lines before the extraordinary epidemic of last year, so that we were pretty well prepared to meet the emergency.

Whenever we have had a case of fever, the car has been thoroughly fumigated with sulphur, then laid by until it was believed that the germs of contagion were entirely destroyed. We are very certain that the disinfectants are thoroughly used in the cars, as the matter is continually being reported upon by our inspectors and special service men, and we are equally sure that our linen is clean on every trip, that the carpets are taken up and well beaten, and the plush and other interior arrangements of the cars thoroughly cleansed. The mattresses are frequently re-upholstered, and the pillows, blankets, etc., are thoroughly cleaned at stated times, and if inspection shows that anything of that nature requires treating oftener, the inspector immediately makes a report to that effect to this office.

In regard to the matter of ventilation, we will be very glad to receive any suggestions that any Sanitary Council may see fit to make. We are constantly experimenting with new ventilators. Most of the so-called inventions, however, are impracticable. Such as we find to be of benefit, we adopt. We are now experimenting with several styles of ventilators, one of which, at least, we hope will be found of some benefit.

In regard to making short runs, we have no doubt that in a sanitary point of view short runs would be better for the passengers, but that is a matter we cannot control. The railroad companies and the traveling public are in favor of long lines. Passengers will submit to a great deal of discomfort rather than change cars, and it is probable that it will be some time before their views will be altered.

Regarding your suggestion that cars should be built for a climate south of lat. 37°, which should consist of entirely different interior fittings to those used in the north, I would say that such a change would entail upon the Southern company a great expense. We made the attempt several years ago to furnish sleepers to a Southern road with hard finished floors without carpets, but on arrival of the cars at their destination, the railroad Superintendent ordered carpets put down at once, as he was afraid his passengers would think they were being provided with second class accommodations.

At the same time, if we had cars fitted solely for southern summer travel, we should be obliged to lay them up in winter, and provide others for northern people making winter trips south. At the present time, with limited travel, the carrying out of your suggestion would be too expensive for us. We would be very glad, for such cars as are used on the southern roads to supply hard wooden floors to them as fast as they go into shop, and not use carpets in summer. This is a matter that we will confer with the southern Superintendent about as early as practicable.

In conclusion, I would say that we will be very happy to hear any suggestions from the Sanitary Council in regard to matters discussed in their convention at Atlanta in which we are interested, and so far as practicable will be pleased to adopt them.

Very respectfully,

D. N. WELCH,

Gen. Sup't P. P. Car Co. and Vice Pres. P. S. Car

PULLMAN SOUTHERN CAR COMPANY,

GENERAL SUPERINTENDENT'S OFFICE,

LOUISVILLE, May 29, 1879.

Respectfully returned to Col. D. N. Welch, Vice-President, Chicago.

I have carefully read the enclosed clipping, from the *Atlanta Constitution*, of the proceedings of the National Board of Health, relating to the yellow fever epidemic of 1878; Dr. Rauch's letter and your endorsement of the 27th inst., referring the papers to me for a report of the measures used by the company to prevent the spread of the disease by means of our cars, etc.

In reply, I would state that early in July of 1878, when the Vice-President and myself were in New Orleans, there were rumors of some few cases of yellow fever, and not a few predictions of an epidemic; and at this early date, to guard against probable emergencies, additional sanitary measures were inaugurated, and the use of disinfectants in our cars was commenced. We sought and obtained the best medical advice as to the use of disinfectants, and before the disease had developed itself we were using every precaution, under a well-defined system; and by the time the tide of travel had set northward, this system was in full operation under rigid discipline.

All our cars were thoroughly disinfected at every terminus of our lines, and pure carbolic acid was exposed in open vessels in every car while en route. At terminal points every car was thoroughly cleaned—all bedding, seats, carpets, and in fact everything movable in the car, were taken out, whipped, brushed, and then fumigated in a close room with sulphur. Each car was scrubbed inside and out, and then closed and fumigated with sulphur. After this process with car and equipments, they were both exposed to the open air for several hours, carefully dusted, and both car and equipments liberally sprinkled with carbolic acid. Carbolic acid was put in each

spittoon on every car running into or through any infected district five or six times during each day or night. All cars were thoroughly ventilated while *en route*, by open doors, windows and deck sash.

As soon as the disease was declared epidemic in New Orleans, Memphis and other points, our cars were withdrawn, as in the case of the lines from New Orleans to Chicago and St. Louis, from Louisville to Memphis and Little Rock, and from Cincinnati to New Orleans, via Montgomery. We had one line of sleepers only that continued unbroken during the entire epidemic—the line from New Orleans to Cincinnati, via Milan. On this line all cars were treated at each terminal point and *en route* as above indicated.

During the entire course of the epidemic we did not have a single case reported to us of yellow fever occurring from any *supposed infection* from our cars; and in *every instance* when any person coming from any infected district was taken sick on one of our cars, *not only the person*, but the *mattress, pillows, blankets and sheets as well, were removed at the first station, and all bedding and material thus removed was invariably destroyed—in no instance was any portion of it ever returned to a car.*

We do not know of a single case, *nor do we believe one can be found*, of yellow fever developing itself on any of our cars, *except in persons coming north from infected districts*, and in whose system, beyond a doubt, the germs of the disease were implanted before they undertook the journey. In every case of this kind (and they were very few), the persons, and everything movable in the car with which they had come in contact, were removed from the car as above described.

As before stated, all of our lines, except the *one* noted, terminating south in infected localities, were either contracted to the northern confines of the district in which the disease had been declared epidemic, or entirely withdrawn.

The one exception, from New Orleans to Cincinnati, via Milan, our cars were cleaned, and all the bedding and equipments therein handled at Cincinnati by men living in that city; and not only no case of yellow fever, but no case of disease of any nature, developed itself among any of these men during the entire summer and fall.

In contracting our lines as the disease manifested itself in Memphis and other points south, in the latter part of July and the early part of August all lines whose northern termini were in this city (Louisville) were entirely withdrawn, and thenceforward, until after the Board of Health had declared it safe for citizens to return to Memphis, *not a single sleeping car coming from an infected district came into this city*; the cars on the line from New Orleans to Cincinnati passed around the city by the short line cut-off, leaving the main stem of the Louisville and Nashville railroad three miles south of the city.

* * * * *

In conclusion, I would say, were steamboats as careful of their equipments and general sanitary condition—*were the traveling public one-half as careful of their clothing and baggage*, to prevent the spread of infectious diseases—as our service requires, I opine there would be less need of such strict quarantine measures; but under the existing regulations, I am in favor of the most thorough investigations, in cases of epidemic, of all persons and things leaving any infected locality. We have always done everything possible to prevent the spread of disease by means of our cars—have done much more than the National Board of Health appears to require; and we would like to see such measures inaugurated as would prevent the transportation of infected clothing, merchandise, etc., as this is undoubtedly the source from which the most danger is to be apprehended.

THOMAS S. HAYS,

General Superintendent.

PULLMAN PALACE CAR CO., CENTRAL DIVISION,
SUPERINTENDENT'S OFFICE,
PHILADELPHIA, June 7, 1879.

Respectfully returned to Col. D. N. Welch, General Superintendent, having noted carefully the within correspondence, and give below a report of the manner of handling cars, bedding, equipments, etc., in Central Division.

All mattresses, pillows, seats and blankets are thoroughly aired, beaten or shook at the termination of each trip, which will make an average of three times per week that they receive this treatment.

As a disinfectant we use a mixture of carbolic acid and copperas, or carbolic acid alone, applying the same only in urinals and closets.

The only case of yellow fever that we had in any car of this division was in sleeping car No. 287 *en route*, St. Louis to Jersey City, September 13-15. A lady passenger, who entered the car at St. Louis, complained of feeling sick, but supposed it to be a case of chills and fever, until the train had arrived at or passed Pittsburgh, when the passengers in the car concluded it was a case of yellow fever. The car went through to its destination, Jersey City, and was immediately switched over into the side or freight yard, the authorities notified, a tug chartered, and the lady and her husband taken down to the quarantine station in New York harbor, some time about three or four o'clock in the morning.

The physician of the Penn. R. R. Co. was given charge of the car, and he immediately disinfected it, which he did in a most thorough manner, as our reports will show, the equipments and all the interior of the car, so far as metal work, etc., were concerned, being almost completely ruined by the use of acids. After they were through with disinfecting the car, it was sent to Altoona and remained in an exposed position during the winter, at a temperature of weather sometimes as low as 13° below zero. The car was then shopped for thorough and general repairs, costing nearly four thousand dollars.

As has been our custom, which we are still pursuing, all cars, at the termination of each and every trip, are thoroughly washed and cleaned, bedding and equipments aired and dusted, carpets taken up and beaten, and as you will see by your pay-rolls, we have quite a small army of cleaners doing this class of work.

E. H. GOODMAN,
Superintendent.

Since the foregoing correspondence I have traveled at least 45,000 miles in sleeping cars. I have paid especial attention to this subject, and have reached the conclusions which follow. The condition of the car depends on (a) the handling of the car by the conductor and porter; (b) the number of passengers; (c) the length of the run; (d) the season of the year.

It is but just to state that the sanitary conditions of the Pullman cars are better than any other cars, and from correspondence and personal interviews I am satisfied that the company would cheerfully adopt the suggestions, if the public demands the same. My excuse for calling attention to the subject at this time is to educate the public to the dangers incident to traveling in through cars.

In 1879 when no cars were allowed to cross the Ohio at Cairo, transfers were daily made, and not only was there no complaint but much commendation of the method. I would respectfully suggest that this Board take such action as may be deemed proper to the subject, feeling confident that the traveling public will cheerfully comply when the benefits are made known and the companies will gladly make the change.

DANGERS OF SUMMER RESORTS.

I was at Beach Haven, on the New Jersey coast, when the Parry House was destroyed by fire. The fire was discovered at 3 A. M. and as the building was constructed of the lightest material, it was completely destroyed within an hour.

It seems almost miraculous with the limited means of egress that no lives were lost. I have been in burning hotels, but never were the inmates so completely at the mercy of the flames as in this instance. As it was, several have since died from the exposure and excitement incident to the fire. This hotel is a type of the scores to be found on the New Jersey coast, and unless steps are taken to require adequate means of escape and facilities for putting out fires, it is only a question of time when great loss of life will occur. In view of the fact that during the season, thousands from other States are guests at these houses, I think it would not be out of place to suggest that our Board call the attention of the New Jersey State Board of Health and the Boards of Health in other States where such resorts exist, to the subject and to respectfully recommend that in case there are no laws concerning it, they make an inspection of these buildings covering these points, as well as the sanitary conditions that obtain in and around these houses. The publication of such a report would go far towards remedying the evil.

The report was discussed, and the following resolutions and orders therein suggested were duly adopted by vote :

WHEREAS, In several instances recently, when suits have been brought against parties for the violation of the "Act to regulate the practice of Medicine," advantage has been taken of the ten-year clause by those who do not strictly come under its provisions ;" therefore,

Resolved, That it is decided by this Board that the ten-year exemption clause, as a qualification, means that the party who avails himself of the said exemption must have actually been engaged in the practice of medicine as a means of gaining a livelihood, and must have publicly professed himself to be a physician for a period of ten or more years before July 1, 1877; further,

Resolved, That parties who have only occasionally prescribed during that time, are not by this Board considered as entitled to the benefits of the exemption.

ORDER.

Under the authority conferred upon the State Board of Health by section 2, of the State Board of Health Act, it is ordered—that on and after Jan. 1, 1882, the first cases of small-pox occurring in any township, town or city, in this State, shall be promptly reported to the Board, and also the prevalence of any epidemic, by the local health authorities, it being borne in mind that in counties where township organization exists, the township board is the Board of Health, and in counties not under such organization, the county commissioners act in like capacity.

Resolved, That the Secretary of the Board be, and is hereby instructed to request the medical schools of the State to give one or more lectures each year on the relations of the profession to the public and to each other, believing the same to be in the interests of the people and of medical men.

Drs. Clark, McLean and Bateman, Committee on Railway Sanitation, reported as follows:

WHEREAS, Earnestly believing that many sanitary evils exist with regard to the management of emigrant trains, ordinary passenger trains, sleeping coaches, the grounds and buildings of depots and even the trains for the transportation of goods and merchandise, therefore,

Resolved, That the Secretary of this Board be instructed to communicate with the managing authorities of the various lines in this State, calling their attention to the matter and requesting them to take such steps as they consistently can with reference to the sanitary improvements in these respects.

John I. Spence, of Liberty, Adams county, having applied for a certificate of graduation, presented the diploma of the College of Physicians and Surgeons of Joplin, Mo. Upon motion of Dr. Clark, the following order was adopted :

WHEREAS, It appearing from the published announcement of Joplin College of Physicians and Surgeons at Joplin, Mo., that the said medical college is not conducted according to the custom of such institutions considered in "good standing," in that it recognizes three years of practice as equivalent to one course of lectures, and that the general character of the announcement is such as to make it evident that it is not conducted in accordance with the practice of medical schools that are considered in "good standing" by this Board ; therefore,

Resolved, That this Board declines to issue a certificate to its graduates.

Three other practitioners actions were taken under advisement by the Board, and in one case discretionary power was given to the Secretary to revoke the license.

Dr. Bateman moved, that the Secretary be authorized and instructed by this Board to prepare and issue as soon as practicable, the necessary blanks, forms, circulars and instructions for the collection and tabulation of returns of vital statistics.

Upon motion of Dr. McLean, the Secretary was instructed to issue a circular calling the attention of college authorities to the requirements of this Board in respect to the good standing of Medical Colleges as published in its report.

The case of Dr. M. H. Pasco, of Chicago, was referred to the Secretary with power to withdraw the Board's certificate at his discretion, and report his action at the next meeting.

Drs. Rauch, Gregory and McLean were appointed a committee to report to the Board at its meeting as to what action, if any, was necessary on the subject of Maritime Quarantine.

The Board then adjourned.

ARTICLE XI.

STEPHENSON COUNTY SOCIETY OF PHYSICIANS AND SURGEONS.

At a special meeting of this society which took place recently at the office of Dr. B. T. Buckley to take action concerning the death of Dr. L. A. Mease, the oldest practicing physician in Freeport, a graduate of Rush Medical College, of Chicago, and Jefferson Medical College, at Philadelphia, the following resolutions were unanimously adopted :

WHEREAS, It has pleased God to remove from among us, L. A. Mease, M.D., late President of this Society and for over thirty-five years a practicing physician in this county.

Resolved, That while we bow with humble submission to the Divine will, we cannot but regret the loss of a pioneer physician, whose death occurs at an age when we might reasonably hope to enjoy, yet for many years, the benefits of his ripe experience.

Resolved, That we recognized in him a courageous and skillful surgeon, fitted by anatomical and surgical knowledge and research, by judgment, coolness and manual dexterity to be, as he was for many years, the leading operative surgeon of this region ; a competent and faithful physician ever ready to respond to the call of rich and poor alike, and to furnish to all the benefit of such skill and experience as, especially in the early days of his practice in this community, were as rare as they were precious to an infant settlement.

Resolved, That we tender to his bereaved family our sincere sympathy in their affliction.

Resolved, That a copy of these resolutions be spread upon the minutes and furnished the family of the deceased.

B. T. BUCKLEY,
F. W. HANCE,
LOUIS STOSKOPF,

Committee.

Domestic Correspondence.

ARTICLE XII.

THE EDITORS OF THE CHICAGO MEDICAL JOURNAL AND EXAMINER.—The patient here referred to was cured of epilepsy by the *enucleation* of an eye. Mr. K., of Indianola, Texas, for years past had been troubled with epileptic fits. The history given by himself to me was, that about ten years ago he lost the sight of one of his eyes by taking "cold" in it. Since that time he would have spells of his eye inflaming, paining him very much and usually bringing on an epileptic fit. I advised its enucleation. I told him that it was possible and even probable that it might be the means of curing him of his epilepsy. He consented to its removal. On the 9th day of February last I removed it. While under the influence of chloroform and while I was in the act of removing the eye, he was seized with a fit of epilepsy and it took four strong men to hold him on the operating table. I kept up the chloroform, and as soon as his fit left him, lasting about ten minutes, I at once removed the eye. With cooling and antiseptic dressing the socket soon healed. I then, as a constitutional remedy, put him on the use of bromide of potassium. He has had no epileptic fit since.

H. ROSENCRANS, M. D.

ELGIN, ILL., August 18, 1881.

THE ninth annual session of the American Public Health Association will be held at Savannah, Ga., Nov. 29 to Dec. 2, inclusive.

Reviews and Book Notices.

ARTICLE XIII.—LECTURES ON THE DIAGNOSIS AND TREATMENT OF DISEASES OF THE CHEST, THROAT AND NASAL CAVITIES. By E. FLETCHER INGALS, A.M., M.D., Lecturer on Diseases of the Chest and Physical Diagnosis, and on Laryngology, in the Post-graduate Course, Rush Medical College; Clinical Professor of Diseases of the Throat and Chest, Woman's Medical College; Physician and Surgeon for Diseases of the Throat and Chest, Central Free Dispensary, Chicago. With one hundred and thirty-five illustrations, pp. 437. Wm. Wood & Co., New York, 1881.

This forms a valuable aid, both to the student and practitioner, in the study of its subject. It is clear and concise in style, systematic and thorough in the consideration of each detail. The author presents the results of wide personal experience in this specialty, and exhaustive study of the literature already before the profession upon the subject.

It comprises thirty-three lectures, the first of which is devoted to an elaborate study of the regional anatomy of the thorax. Lectures II to IX contain the methods of examination of the chest in health and disease; those on inspection (and under this head are found many useful hints to the student), palpation, mensuration, succussion, percussion and auscultation. The analysis of the two latter we think particularly fine. The elements of the sounds obtained by percussion in health and disease are described as to intensity, pitch, quality and duration, in every variety of condition, from clear vesicular resonance to absolute flatness, and precise rules prescribed to the student in percussion. In auscultation the same elements are considered, with the addition of

rhythm, through each modification of the respiratory act, from the soft breeze of the vesicular murmur to the loud blowing of the trachea.

Special care is enjoined throughout upon the student to become familiar with the sounds of the healthy chest; "to form an ideal from which no great variation can occur without indicating disease." Lectures IX to XV comprise the Diagnosis and Treatment of Pulmonary Diseases, and we find that the thorough preparation in the preceding lectures leads up to the grouping of the symptoms and signs of each disease in clear and lucid style. The differential diagnosis of each disease receives great care, and tables are appended wherein the essential difference in the signs of diseases liable to be confounded with each other are distinctly shown under each method of examination.

Lectures XV to XX comprise the Anatomy, Relations and Physiology of the Heart and Aorta. The methods of cardiac exploration each receive careful detail, with the information obtained by it; and here we notice, under the head of inspection, a table illustrating the significance of alterations in the situation of the apex beat, which conveys a good deal of information in small space.

The sounds of the healthy heart and their modifications receive due consideration under auscultation. Two chapters are given to abnormal sounds of the heart and their differential diagnosis. The cuts illustrating this subject are clear and plain. Lectures XX, XXI and XXII present a complete exposition of cardiac diseases and the diseases of the aorta, and here, under each head, the confrontation tables contribute to its clearness.

The remainder of the book is devoted to the fauces, larynx and nasal passages. Four chapters on laryngoscopy and rhinoscopy, description, difficulties of and rules for, and the anatomy and physiology of the larynx and post-nasal space, followed by the diagnosis and treatment of all the diseases of the throat and nasal cavities known to present science.

The chapters on the larynx are especially well illustrated. The volume is well furnished with table of contents and index, and a handy list of formulæ is appended, in which are found the remedies used by the author, with others compiled from various sources.

When these lectures were delivered the subject of treatment was not mentioned, but in order to enhance their value in this form, the author has appended to the consideration of the diagnosis of each disease an outline of the treatment found by him most satisfactory, without mention of many different methods recommended by others.

The special points to be commended in this work are, thoroughness of consideration, method of arrangement for study, and availability of the contents for reference.

W. C. D.

THE first meeting of the State Microscopical Society of Illinois for the present season was held at the rooms of the society in the Academy of Sciences, Friday evening, Oct. 14, the President, Dr. Lester Curtis, in the chair.

After the transaction of routine business, Mr. Stuart described the microscopical structure of some vegetable drugs. The subject is not suitable for abstraction, and requires illustrations to be useful.

His paper was followed by one by Dr. Curtis, describing a new stand made for him by Bulloch. This stand presented some novel features. Among the most striking was a mechanical stage of extreme thinness, admitting light at an angle of 160° . The movements were effected by a double pinion above the stage, an arrangement pronounced by those familiar with the operation of the contrivance as exceedingly useful and convenient.

The stand excited considerable attention, as did also a right angled camera lucida of German manufacture which was adapted to it; the superiority of which over the ordinary form was so marked as to be unmistakable on trying it, even under the disadvantages of a crowded room and constant jar. After a discussion of the papers, the meeting adjourned.

E. B. STUART, *Secretary pro tem.*

Summary.

QUARTERLY ABSTRACT OF OPHTHALMOLOGICAL AND OTOLOGICAL LITERATURE. By Dr. E. J. GARDINER, Chicago.

ON THE CONTINUOUS ELECTRICAL CURRENT AS A THERAPEUTIC AGENT IN ATROPHY OF THE OPTIC NERVE, AND IN RETINITIS PIGMENTOSA. By R. Marcus Gunn, M.A., M.B.—(*Ophth. Hosp. Rep.*, Vol. X., P. 2.)

Method of application.—Weiss' continuous current battery of 25 cells was used, fitted in the ordinary way with sponges, which were moistened in a solution of common salt. On employing it for the first time in any case, the anode was applied lightly over the closed lids of one eye, and the cathode on the opposite eye or temple. The power was gradually increased, beginning with five cells until a distinct flash was experienced by the patient, on making or breaking the circuit. The anode was kept over the eyelids, while the cathode was applied in turn to the top of the spine, to a point just behind the mastoid process, and to the supraorbital region of the same side, so as to determine which position gave the greatest light impression. This varied in different individuals, but in the majority of cases, the supraorbital region was selected.

This position (the supraorbital) being determined, the cathode was kept there for half a minute or so, then removed for a few seconds, and finally reapplied for the same period as before. It was next moved to the corresponding point on the opposite side of the head, and then to the closed eyelids of this side. In both these positions it was applied for a few moments. Both poles were now removed, and the cathode then applied where the anode had been, while the anode was applied to the supraorbital

region, etc., as above. The same course was then gone through with the other eye, a slightly stronger current being finally used for a few seconds. The whole sitting lasted from five to eight minutes.

G. reports eighteen cases of atrophy of the optic nerve, treated in this manner. The following results were obtained: In case II, improvement doubtful; in cases I and X, temporary improvement; in cases V, VI, VII and XI there was marked improvement. All the other cases showed no improvement.

The continuous electrical current was tried in four cases of retinitis pigmentosa, with most satisfactory results. Case I when admitted $V = \text{in R. E.} = \frac{3}{200}$; $\text{L. E.} = \frac{20}{200}$ with—36 $V = \frac{20}{70}$; when discharged May 10, $\text{R. E.} V < \frac{20}{70}$ with—40 $V = \frac{20}{70}$. $\text{L. E.} V = \frac{20}{40}$.

Case II.—Admitted Nov. 9, 1879. $V = \text{Jaeg. 4}$, barely with each. Dec. 20. Reads Jaeg. 1 fluently with R. E. barely with L. $V = \frac{20}{100}$ barely with each.

Case III.—Admitted Oct. 6, 1880. $\text{R. E.} V = \frac{20}{100}$ with 2'0 D $\frac{20}{70}$ Jaeg. 1 at 12". $\text{L. E.} V = \frac{20}{70}$ with—1'0 D. $\frac{20}{40}$ Jaeg. 1 at 12". Discharged Dec. 20.

$\text{R. E.} V = \frac{20}{70}$: with—1'50 D $= \frac{20}{30}$ partly.

$\text{L. E.} V = \frac{20}{70}$: with—1'50 D $= \frac{20}{30}$ partly.

In Case IV the results were very satisfactory, but the amount of sight is not recorded.

A CASE OF SIMULTANEOUS SUBCONJUNCTIVAL DISLOCATION OF BOTH CRYSTALLINE LENSES, CAUSED BY THE KICK OF A HORSE. By J. C. Wordsworth, F.R.C.S.—(*Oph. Hos. Rep. Vol. X, No. II.*)

A man, æt. 68, eight weeks before admission, had received a kick from a horse on his face, he was rendered unconscious for a few minutes. The bones of his nose were fractured, and both eyes were ruptured. "At the time of his admission, each crystalline lens was lying under the conjunctiva, at short distances from the cornea, a little above, and to the inner side of its margin—having escaped, through a laceration in the scleral coat of each eye, concentric to the upper and inner edge of the cornea." Good anterior chamber in both eyes; corneæ clear; pupils drawn upward and occupied by dark clots; eyes almost free from pain;

Six weeks after admission, the lenses were removed by division of conjunctiva over them. Very little irritation followed. Five months after injury was examined for glasses, when his vision was as follows :

R. E. with 10'50 D. V $\frac{20}{20}$ with 15 D. V=Jaeg. 1 at 7".
L. E. with 10'50 D. V $\frac{20}{40}$ with 15 D. V=Jaeg. 1 ; slowly.

A CASE OF PRIMARY EPITHELIOMA OF THE LOWER EYELID.

By George Lawson, F.R.C.S. (*Ophth. Hos. Rep. Vol. X, P. 2*).

A man of 65 presented himself suffering from a primary epithelioma of the lower lid. He stated that it began as a small pimple, about three months previous it ulcerated and rapidly increased until it attained the present dimensions. Vertical diameter a little over three centimetres, horizontal diameter about three centimetres. It involved the greater part of the margin of the lower lid—it was a distinctly new growth—very solid when felt between the fingers—it could be raised from the subjacent tissues. The margin was hard and rounded, and the whole surface of the growth presented a coarse ulcerated surface, covered in parts with thick yellow purulent secretion. L. completely excised it, leaving an open wound to granulate and cicatrize. The patient has continued well since.

THREE CASES OF NEW FORMATION OF BLOOD-VESSELS IN THE VITREOUS. By W. Charney, M.A., M.D.; and L. Webster Fox, M.D. (*Ophth. Hosp. Rep., Vol. X, P. 2*.)

Case I.—A young girl, æt. 14, a year ago noticed a black thread before her eye. On careful examination, the R. E. exhibited some small vessels which could be traced backwards to the lower margin of the discs, and which anteriorly formed a succession of loops and seemed to terminate near a glistening white mass in the vitreous.

Case II.—A girl æt. 19. R. E. Keratitis punctata—signs of previous iritis. In the L. E. was seen a floating vessel of a pale red color and peculiar spiral course, which could be traced forward to near the equator bulbi, then turning upward and outward, was lost in the vitreous ; neither its point of origin or ending could be

ascertained owing to the haziness of the media. While under treatment (mercurial) the caliber of these vessels gradually lessened, and eight months after the date of admission of the patient, they had entirely disappeared.

Case III.—Woman, æt. 51. R. E. downward from the disc was seen a grayish spot of connective tissue on the retina, from which a bunch of vessels extended upward and forward into the vitreous. Some presented free extremities, while others formed closed loops, returning to their place of origin, after having at their most anterior part formed a very fine and close spiral. They floated in the vitreous, moving with the motions of the eye.

VOLUNTARY NYSTAGMUS. By George Lawson, F.R.C.S. (*Opth. Hosp. Rep., Vol. X, P. 2.*)

L. reports the case of a gentleman who at will could set both his eyes into a rapid lateral motion. The movements were an exaggeration of what is seen in lateral nystagmus, they were so rapid however, that the margins of the cornea could not be defined.

Des Indications de l'Iridectomie et de la Sclerotomie dans le Glaucome. Par le Docteur Ch. Abadie. Ann. d'Ocul. Mai, Juin.

After reporting a few cases in which iridectomy was performed with no permanent relief to the patient, A. concludes that sclerotomy should be performed.

1st. In hæmorrhagic glaucoma.

2nd. In chronic simple glaucoma, characterized by the excavation of the disc, with no disturbance of the media, and by slight increase of the intra-ocular tension.

3rd. In cases of congenital hydrophthalmus with increase of tension and in which, owing to the deformity of the ciliary region, the excision of the iris may be followed by the rupture of the zonula, luxation of the lens, prolapse of vitreous, etc.

4th. In those rare forms of glaucoma which are characterized by a fatally progressive course, and which seem to continue in spite of the iridectomy. The advantage of sclerotomy in these

cases, is that it can be often repeated until a pervious scar is obtained.

Iridectomy should be performed in those cases which exhibit a shallow anterior chamber, and where the iris and the lens are pushed forward.

TWO CASES OF EXTRACTION OF SPLINTERS OF IRON FROM THE VITREOUS. WITH OBSERVATIONS ON THE DIAGNOSIS AND EXTRACTION OF STEEL AND IRON PARTICLES BY MEANS OF THE MAGNET. By Dr. H. Pagenstecher, of Wiesbaden. (*Arch. of Opth.*, Vol. X, No. 2.)

Case I.—A man, æt. 41, was struck upon the left eye by a piece of iron, ten hours before presenting himself. On the inner sclero-corneal margin there was a wound corresponding to which there was a wound in the iris. The iris was fastened to the wound; lens clear. On the 4th day, a yellowish nodule was seen projecting into the vitreous. As this indicated precisely enough the direction of the foreign body, the extraction was at once performed. An incision was made in the inner and lower part of the sclero-corneal margin, the corresponding portion of iris was cut out. A fine, bent probe was then introduced—between the lens and ciliary body—and the foreign body was touched; then a pair of simple tooth forceps were introduced in the same direction, and the foreign body having been grasped at the first attempt was removed. The border of the lens was wounded in the process of extraction. Vitreous did not escape. Healing took place without much reaction, though the opacity of the lens slowly increased. The cataract was removed five months later. With + 3, fingers were promptly counted at three meters.

In the 2nd case, Frölich's electro-magnet was used. The patient was a mason, 28 years old, who while breaking stone with an iron hammer, was struck by something on the left eye—Stat. Pres. Perforating wound of upper lid and corresponding to the same, a small linear one near the inner corneal margin in the sclera; cornea, iris and lens intact; blood on the floor of the anterior chamber. In the middle of the vitreous was seen a pretty large air-bubble, and downward and outward there was a

striped opacity in the vitreous which terminated in a considerable hæmorrhage on the floor of the vitreous ; pain during the night. Next day cornea and aqueous hazy ; iris swollen, deeply injected and painful. On the 4th day panophthalmitis seemed likely to ensue. With patient fully under the influence of chloroform, P. made a small incision in the sclera, in the situation of the previously observed blood clot. Through this the sound of the electro-magnet was introduced and moved around. A loud click of the foreign body against the sound was heard. Two ineffectual attempts were made to extract the foreign body ; finally, having enlarged the sound, a rough angular splinter of iron two millimetres wide and one millimetre thick was drawn out. A slight loss of vitreous tinged with blood followed ; wound was closed with antiseptic bandage. Everything continued well for a few days, but soon a lingering irido-choroditis set in, which led to the shrinking of the globe. For locating the foreign body P. recommends the use of a magnetic needle such as described by Pooley, only Pagenstecher allows the needle to rotate as in a compass on a fine needle. Attention is drawn to the fact that if the media are clear and the foreign body is movable, in rendering the eye magnetic by touching it with the electro-magnet, the foreign body will be seen to follow the movements of the magnet. If this experiment fails, it is likely that the foreign body is imbedded in the tunics of the eye, and then the examination with the magnetic needle is to be resorted to. The experiments performed upon rabbits, with Frölick's sound were generally successful ; if the reaction produced by the foreign body was not too great. With regard to the operation in man the prognosis is not so favorable. P. says, however, that "the introduction of the magnet for the purpose of removing fragments of steel and iron from the eye, and for their diagnosis, is a considerable step in advance, and we are now in a condition to save many an eye which would otherwise irreparably be lost."

CLINICAL CONTRIBUTIONS. By Dr. E. L. Holmes.—(*Arch. of Oph.*, Vol. X, No. II.)

A Case of Aneurismal Tumor of the Orbit. The patient, a lady of 40, was suddenly seized with a most violent pain in left orbit. An enormous exophthalmus at once followed.

When H. saw the patient auscultation and palpation revealed the sound and impulse peculiar to aneurism—redness and œdema of exposed ocular conjunctiva; cornea had lost its sensitiveness, it was only kept moist by artificial means, and showed signs of speedy and complete necrosis; condition of lens, vitreous and fundus could not be determined. It was deemed best to ligate the carotid artery. After two days, however, the aneurismal symptoms diminished. Five weeks later the sounds had spontaneously ceased, with great decrease of swelling and pain. At the end of sixty days after attack, the lids rested partially closed over an atrophied globe. Two years later the patient died suddenly at the breakfast table, without the slightest premonition.

Case of Persistent Hyaloid Canal.—A boy, 17, presented himself suffering from a high degree of amolyopia in the R. E. Examination revealed “a remarkable white object, resembling a wine bottle, with its body, neck and head reaching forward almost to the center of the post. cap. of the lens.”

A Case of Traumatic Aneridia.—A man, æt. 53, had received (two months previous to examination) a severe blow on the right side of face and eye. No traces of iris could be found. With a proper direction of the light from the mirror, a clear, dark ring could be seen enclosing a black ring (the periphery of the lens) within which was the ordinary red reflex. The ciliary processes were not visible as regular elevations, but simply as a dark ridge just behind the border of the cornea; changes of form of the lens and of the ciliary processes could not be discerned during efforts of accommodation. There was no trembling of the lens.

ON QUININE AMAUROSIS. By Dr. H. Knapp.—(*Arch. of Opth.*, Vol. X, No. 2.)

Blindness was produced in the three cases by quinine poisoning. The restoration of central acuteness of vision was complete in first case. It was incomplete in the second and third. K. considers the prognosis favorable even in advanced cases, there being thus far no case of permanent blindness on record.

DU TRAITEMENT DE LA KERATITE INTERSTITIELLE, ET DE LA SCLERO-KERATITE.—Par l'Iridectomie ; par le Dr. Galezowski. (*Rec. d' Opth.*, series 3, No. 7.)

G. thinks that in those cases of interstitial keratitis where the iris becomes implicated, iridectomy should be performed. He reports several cases in which this operation stemmed the disease when other treatment had failed to do so. His reason for operating is that, "when the iris becomes adherent to the capsule after a time it disturbs the circulation in the anterior lymphatic canals, thus depriving the cornea of its nutrition."

AN OPERATION FOR PROMINENCE OF THE AURICLES. By Dr. Edward T. Ely.—(*Arch. of Otol.*, Vol. X, No. 2.)

The description of the operation being a short one I will quote Dr. E. "An incision was made through the skin along the entire length of the furrow formed by the junction of the auricle with the side of the head posteriorly. This was joined at each end by a curved incision carried over the posterior surface of the auricle, and the skin and subcutaneous tissue included by these incisions were dissected off. Two incisions, nearly parallel to the prominence were then carried directly through the cartilage and an elliptical piece of the latter measuring $1\frac{1}{8}$ inch by $\frac{1}{2}$ inch, was removed. The piece of excised skin was considerably larger than this. The edges of the wound were then united by ten sutures, of which seven included only the skin, while three passed through both skin and cartilage. Owing to the natural folds of the cartilage, it was impossible to secure perfect coaptation of the anterior surface of the auricle, and a small piece was here left to heal by granulation. The dressing consisted of absorbent cotton and a bandage. . . . Healing ensued without accident.

BILATERAL RUDIMENTARY AURICLE WITH ABSENCE OF THE EXTERNAL AUDITORY CANAL. By Dr. H. Knapp.—*Arch. of Otol.*, Vol. X, P. 2.)

A man, age 30, of healthy parentage, was born with a rudimentary auricle on both sides. "The auricle presents a curved, crook-like ridge of skin, which in the upper curved half contains cartilage; whereas the lower half, which has the feel of the lobule,

consists only of skin. Behind the rudimentary auricle there is a flat, round depression of half an inch in diameter, bordered posteriorly by a well developed mastoid process. Placing the finger in the depression, the movements of the lower jaw are felt as distinctly and in the same manner as when the finger is placed in a healthy auditory canal. The depression behind the ear therefore, corresponds to the external meatus." The whole head, in particular the pharynx, nose and jaws are well developed. A catheter can be introduced into the tubes and the stream of air will be heard to penetrate into the tympanic cavities, when a stethoscope is applied both in front and behind the rudiment of the auricle. . . . He understands loud voice with some difficulty at the distance of five or six feet. A physician of Philadelphia, thirteen years ago, unsuccessfully attempted to open the passage.

GOWERS ON PARALYTIC CHOREA.—Five cases in children of ages varying from seven to fourteen years, three of whom were girls, formed the basis of a paper read by Dr. Gowers at last year's meeting of the British Medical Association. They illustrate that form of chorea in which the muscular weakness is the prominent symptom, the twitching and inco-ordination of voluntary movements being slight. In regard to such cases, Dr. Gowers remarks, rarely slight clonic spasm occurs in the affected arm at first, and subsequently passes off. The loss of power may be very great, as it may be much less than the loss of use of the limb would suggest. In these cases there is inaction rather than paralysis; usually one arm only is affected, and there is no paralysis of face, tongue or leg. Both arms may be affected, but one is always the weaker. On careful watching, clonic twitching is to be observed, though often very slight. The course of such cases is very tedious, but they have not been seen to pass into severe general chorea. The reported cases all recovered under the use of liq. strychniæ.—*London Medical Record*.

Items.

CARIATIDES FROM CADAVERS. — An engineer of Havana, named Cruz, proposes the following way of embalming. The body is placed in a bath composed of equal parts of lime and clay, dissolved in a sufficient quantity of water, then it is covered with another layer of natural cement, destined to absorb the excess of water, after which the cadaver is submerged in a bath of pitch, and covered finally with a layer of lime; the contact, only, between the lime and calcareous cement being sufficient to solidify the pitch rapidly; a thick coating being formed in this manner, which possesses the same properties as the pitch of Judea, a substance to which the Egyptian mummies owe their peculiar indestructibility. As can be readily understood, a subject so prepared can exhale no marked odor; the different layers of lime, clay and pitch forming around it a kind of solid wrapping, which is opposed effectually to the disengagement of gases. A cadaver, after being treated in this manner, is deposited in the interior of a mould, which is filled with the following mixture, that very soon solidifies, and is transformed into stone: Cement, five parts; sand, three parts; ashes, two parts; water, a sufficiency. The stones which are obtained by this process acquire a remarkable solidity. Obituary inscriptions can be engraved upon them; they can be placed in mausolea, or may serve for the construction of sepulchral monuments of various forms: (Boston Med. and Surg. Journal.) We would suggest silicate of potassium (liquid glass) as a coat, which would be highly artistic, and through this process lawns and gardens could be adorned with the well-preserved persons of ancestors, in place of common statues. Or, if preferred, the busts of grandparents could be in

the same manner preserved and utilized as mantel ornaments, which would enhance the veneration of the departed.

STATE MEDICAL SOCIETY JOURNALS. — Under the title of *Ohio Medical Journal*, the *Ohio Medical Recorder* has become the journal of the Ohio State Medical Society, and will in future take the place of the usual volume of transactions. The editor in charge is the secretary of the society, and was formerly managing editor of the *Recorder*; he is to be assisted by four associate editors from as many of the larger cities in different parts of the State. We shall watch over the career of our contemporary with special attention. Although long suggested, this is the first experiment of the kind in this country. The disadvantages are numerous, if they do not outweigh the advantages of such an arrangement. There is certain discredit thrown on medical societies by those who prefer not to belong to any, and they are numerous, while they all take at least one medical paper. The editorial department soon weakens, for one reason or another, or may not exist at all, as with the *British Medical Journal*. Cosmopolitan summary of medical science and progress, which is just what the majority of readers need, finds nobody to attend to it. At the same time, a large amount of trash and useless material fill up the pages of the periodical. In journals, as in other literary undertakings, individual efforts generally succeed the best.

LEPROSY IN LOUISIANA.—The annual report of the Louisiana Board of Health for 1880, just issued, contains a detailed statement of the progress of Asiatic leprosy in that State during the last century. It was brought in 1680 to the West Indies, by the negro slaves, and thence to Louisiana. In 1778 this disease was so prevalent among the blacks, together with the African elephantiasis, and another equally horrible, named yaws, peculiar to Guinea negroes, that a hospital for lepers was established in New Orleans. At the present time, the majority of lepers in that city are found to be whites, of French, German and Russian extraction. Although the disease is infectious, New Orleans, it appears, has no separate asylum for these incurable patients, and they

are received into the Charity Hospital, and placed in the crowded wards, to scatter death : (Medical and Surgical Reporter.) Another nidus of Asiatic leprosy exists near Shediack, New Brunswick, where an asylum has been in existence ever since the French domination. It seems that the first cases of leprosy came to that place from Norway.

A SERIES OF PRIZE QUESTIONS.—The prize questions which are proposed by the various medical bodies throughout the world may be taken to indicate the points on which information is most desired. The Royal Academy of Brussels announces a prize to be awarded in January, 1884, of \$1,600 for the best explanation of the pathology and therapeutics of the diseases of the nerve centers, especially epilepsy, illustrated by clinical data and experiments. If the essayist discovers a successful treatment for epilepsy, he is to receive in addition \$5,000.

Other prizes offered by the Academy are for essays on the questions :

“What is the nature of the influence of innervation on the nutrition of tissues ?”

“What part do living germs play in the etiology of diseases ?”

“What effect does drying, as a means of preservation of plants used in medicine, exercise on their medicinal properties ?”

Still another prize (\$240), especially designated as for *international* concurrence, is offered by the same body for the best dissertation on the subject—“The value of spectral analysis for forensic medicine and sanitary science, exhibited by original experiments and investigations.”

The Academy of Medicine of Turin, Italy, offers a prize of \$4,000 for the best essay “On the physio-pathology of the blood.” It is open to the world, but must be either in the Italian, French or Latin tongue.

In Paris, the Société de Chirurgie has lately announced three prizes for competition this year. “The subjects are : “The remote results of ovariectomy ;” “The etiological rôle of contusions in the development of neoplasms.” “The history and theory of union by the first intention.”

The American Neurological Association offers a prize of \$500,

to be known as the "William A Hammond Prize," and to be awarded at the meeting in June, 1882, to the author of the best essay on the "Functions of the thalamus in man;" and the Warren prize committee offer a premium of \$400 for the best essay on "Chronic Bright's disease."

Following are the prizes proposed by the French Academy of Medicine for 1882:

Academy Prize.—Generalized arterial atheroma, and its influence on the nutrition of the organs,—\$200.

Baron Portal Prize.—The lymphatics under a pathological point of view,—\$400.

Mme. B. de Civrieux Prize.—Researches respecting the causes of locomotor ataxia,—\$400.

Dr. Capuron Prize.—The lochia in a normal and in a pathological state,—\$400.

Dr. Godard Prize.—The best work on internal pathology,—\$300.

Dr. Desportes Prize.—The best work on practical medical therapeutics,—\$400.

Mme. H. Buignet Prize.—The best work on the application of physics or chemistry to the medical sciences,—\$300.

Dr. Orfila Prize.—Veratrine, sabadilline, black hellebore and veratrum album,—\$800.

Dr. Itard Prize.—Best work on practice,—\$600.

Mr. and Mrs. St. Paul Prize.—\$5,000 for a specific for diphtheria.—*Bulletin de l'Académie de Médecine.*

NOTES ON ANTHROPOLOGICAL STUDIES.—The question of the size and height of men receives a careful study in the *Verhandlungen* from Herr Ranke and others. It has been supposed by some that the earlier races were larger in size, by others that they were smaller than the people now occupying the same territory. Herr Ranke demonstrates, from a large number of measurements, that man is a creature of the soil on which he lives (?); that certain localities produce on an average men of greater stature than other localities, and that as a general rule, elevations above the sea level and mountainous regions are peopled by the larger races. No diminution in stature, nor any increase, can be

proved to have taken place since even pre-historic times in Europe. The skeletons from the oldest tumuli occasionally show individuals to have reached the astonishing height of two meters (6 ft. 6 in.), but as a rule are not in excess of the stature of the population now dwelling on the same soil.

A REMARKABLE MODE OF ZOÖLOGICAL DISTRIBUTION.—M. Méguin lately presented to the Société de Biologie a collection of fifteen leeches, of a species foreign to those found in France. They were adhering to the walls of the mouths of horses just returned from the Tunis campaign. They had been camping near Bizerte. All the brooks of Northern Africa contain that kind of leeches, called *hæmopsis sanguisuga*, whose jaws are too weak to bite the skin, so that they enter into the animals' mouths when these come to drink, and cling to the mucous membrane. They may live in this situation for weeks, causing hæmorrhages, and even asphyxia, if they enter the larynx. But they die almost immediately when introduced into the stomach or the rectum.

At the meeting of the Société d' Anthropologie, held June 2, 1881, Dr. Magistos exhibited an hermaphrodite, born in 1841, and christened Ernestine Guériau. About the thirteenth year there had been some slight menstruation followed by enlargement of the breasts. When seventeen and a half years old this individual married a man, but the sexual intercourse was not quite satisfactory as it had a pseudo-vagina not very deep, and a penis susceptible of incomplete erection. Husband died in 1871. After this the hermaphrodite had intercourse with women, and owns that it had them before the husband's death also. Height 1 m. 78; black hair, shaves every other day; form is masculine though the voice is feminine; breasts are covered with hair. The pubis is covered with hair, and so is the perineum. There are two labia continuous above with a penis the size of a child's, 4 or 5 c; the glans is imperforate, and there is no prepuce. It is curved with concavity downward when excited. The left labium is larger and contains a testicle of normal size, the left contains a smaller one. At the base and inferior part of the penis is the meatus urinarius whence flow the urine and the sperm.

SOCIETY MEETINGS.

Chicago Medical Society—Mondays, Nov. 7-21.

West Chicago Medical Society—Mondays, Nov. 14-28.

Biological Society—Wednesday, Nov. 2.

CLINICS.

MONDAY.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Prof. Holmes; 3 p. m., Otological, by Prof. Jones.

Mercy Hospital—2 p. m., Surgical, by Prof. Andrews.

Woman's Medical College—2 p. m., Dermatological and Venereal, by Prof. Maynard; 3 p. m., Diseases of the Chest, Prof. Ingals.

TUESDAY.

Rush Medical College—3 p. m., Dermatological and Venereal, by Prof. Hyde.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Quine.

WEDNESDAY.

Chicago Medical College—2 p. m., Eye and Ear, by Prof. Jones.

Rush Medical College—2 p. m., Medical, by Dr. Bridge; 3 p. m., Ophthalmological and Otological, by Prof. Holmes; 3:30 to 4:30 p. m., Diseases of the Chest, by Dr. E. Fletcher Ingals.

THURSDAY.

Chicago Medical College—2 p. m., Gynæcological, by Prof. Jenks.

Rush Medical College—2 p. m., Diseases of Children, by Dr. Knox; 3 p. m., Diseases of the Nervous System, by Prof. Lyman.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Dr. Hotz.

Woman's Medical College—3 p. m., Surgical, by Prof. Owens.

FRIDAY.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Davis.

SATURDAY.

Rush Medical College—2 p. m., Surgical, by Prof. Gunn; 3 p. m., Orthopædic, by Prof. Owens.

Chicago Medical College—2 p. m., Surgical, by Prof. Isham; 3 p. m., Neurological, by Prof. Jewell.

Woman's Medical College—11 a. m., Ophthalmological, by Prof. Montgomery; 2 p. m., Gynæcological, by Prof. Fitch.

Daily Clinics, from 2 to 4 p. m., at the Central Free Dispensary, and at the South Side Dispensary.

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Original Communications.

ARTICLE I.

ADDRESS delivered before the Chicago Gynæcological Society, at the Annual Meeting, September 30, 1881, by DELASKIE MILLER, M.D., President.

GENTLEMEN,—In obedience to the requirements of the rules which govern our society, I proceed to discharge one of the last duties devolving upon me for the current year.

In passing to the consideration of topics which I desire to present to you on this occasion, I do but reflect the sentiments of each member of the society, when I give expression to the feelings of sincere pleasure and devout thankfulness for the convalescence of one of our most esteemed brothers, who, more than a year ago, while in the enjoyment of robust health, and engaged in the active duties of the profession, was suddenly stricken down, and for a long time lay near the border line of the shadow land. We recall with pleasure the many virtues and the kindly disposition, the courteous bearing and the professional integrity which have ever characterized Dr. T. D. Fitch in his intercourse with

members of the profession. While we deeply sympathize with our brother in his severe trial, we at the same time desire to express our thankfulness for his recovery and prospective restoration to usefulness in society, and to the enjoyment of social and domestic happiness for many years to come.

With your permission, gentlemen, I will devote a portion of the time at my disposal, to some impressions of the International Medical Congress of 1881.

On several accounts this congress was a remarkable one. First, on account of the numbers in attendance, which were greater than at any similar meeting of the profession which was ever convened. Second, the delegates represented a broader extent of the earth's surface than any other, including, as it did, representatives from every country where medicine is cultivated as a science. And third, among these delegates, moreover, were a large proportion of physicians who had made more than a local reputation. There were many whose names will ever be associated with the real and lasting improvements in medical science and art, which have been so numerous and so important during the time of the present generation.

The interest of the congress was not diminished by the fact that it was held under the patronage of Her Majesty the Queen, whose interest in every movement calculated to improve the race, and ameliorate suffering, is so well known throughout the civilized world. The proceedings were opened by his Royal Highness the Prince of Wales in person. The address of H. R. H. was a genuine surprise to most of those who composed the vast audience, for its pertinency to the occasion, and the admirable manner of its delivery—the Prince proving himself not only familiar, but also in hearty sympathy with the objects of the congress.

The opening address by the President, Sir James Paget, was an admirable production—comprehensive in scope, elevated in tone and dignity, presenting the aims and usefulness of the profession in a manner that stimulated every heart in that august assemblage with feelings of a just pride in our philanthropic profession.

I am sure I hazard nothing in asserting, that no meeting of the profession was ever held, ever since the world began, that would

compare with this initial meeting of the Congress, in the effects produced upon the minds and emotions of the audience.

It would be inappropriate on this occasion to attempt, as it would be impossible to give even an epitome of the transactions of the different sections—fifteen in number—from personal observation. The proceedings will in due time be published, and will constitute a valuable addition to the medical literature of the current year.

To the section of obstetrics, Prof. Tarnier exhibited his latest improved obstetric forceps, modified in form from that first invented; he having found the instrument in its original form difficult to apply in certain positions of the head, and inefficient in action, except under favorable conditions. He claims for the instrument in its present form (minus the perineal curve), certain advantages over the ordinary double-curved forceps, which may be summarized as follows, viz. :

1st. Greater facility of application in certain difficult cases.

2d. A firmer grasp of the head.

3d. Permitting the movements of the head to take place in its passage through the pelvis, without the guiding aid of the accoucheur.

4th. Simplifying the application of force in extraction.

5th. That it is constructed on scientific principles.

Several eminent obstetricians commended the instrument as an improvement over the ordinary forceps. However, if I am not mistaken, the sentiment of the section, as expressed in the discussion, was decidedly averse to the claims of Prof. Tarnier. For it was contended, on the other hand, that even if the instrument of Prof. Tarnier was constructed on scientific principles, it was still imperfect, and liable to do injury in its use. It was asserted that greater difficulties would be encountered in its application than in the use of the ordinary forceps. Notwithstanding its vise-like grasp of the head, which is liable to injure by its continuous pressure, it is quite as liable to slip as the ordinary instrument. Loosening of the blades and relief of pressure at short intervals, are essential in forceps delivery. And again, the force in extraction should always be directed by the intelligence of the operator, as its direction should vary constantly as the head

passes through the pelvis, and this change of direction is appreciable only by the information derived from the muscular sense while operating.

The subject of oöphorectomy was presented in two papers. One by the gentleman whose name has become synonymous with the operation—Battey's operation.

In the first paper, the object of the operation was claimed to be "The Artificial Induction of the Change of Life." This was assumed to be the pivotal principle that should govern the surgeon in determining the propriety of the operation in any given case.

It was asserted in the second paper, and confirmed in a statement of one gentleman in the discussion which followed, that menstruation sometimes continued after the operation had been performed. To explain this anomaly, one speaker claimed that menstruation was due, not to ovarian excitement, but to the influence of the Fallopian tubes! Are we justified, then, in accepting the philosophy upon which the operation is based if the function peculiar to the sex may continue unmodified after the operation?

It was claimed, moreover, by the advocates of the operation, that it was a remedy for insanity, for in two cases adduced, the patients became sane after the operation, and one gentleman suggested that this operation was destined to become recognized in state medicine, and that insane women should be subjected to the operation, to preclude the possibility of reproduction! On the other hand, however, cases were cited in which the patient became insane after the operation had been performed, in whom no symptoms of insanity had appeared before.

By some who professed to have had much experience, the operation was considered a curative means in severe hæmorrhage from myoma of the uterus. In the neuroses the result of the operation had been less satisfactory than in some other cases. The table of cases operated upon for this complication (hæmorrhage), which was circulated in the section, showed a mortality far greater than has ever occurred from the disease in the practice of any single physician who has treated such cases on the recognized principles applicable in such complications.

The operation was claimed by some who took part in the dis-

cussion, to be one of easy execution, even by the vagina, while another who had operated several times in this way, had relinquished this mode on account of its difficulty, and announced that hereafter he should operate by the supra pubic method. Again, one of the most distinguished ovariologists stated that the operation was far from simple or of easy execution, being, in his opinion, more difficult than ovariectomy; to which statement there seemed to be a general assent.

Again, it was more than hinted by some who had attained the highest eminence in the profession, that the operation had been resorted to with unjustifiable frequency, and not as a *dernier ressort*, as it always should be. All other means should be intelligently applied, and for a time sufficient to demonstrate their inefficiency, before deciding upon this operation. A practical hint of importance is contained in the fact, that in many cases in which the operation has been recommended, the evidence of great suffering is derived from the statement of the patient herself, and is mental or emotional, having no direct dependence upon the sexual organs.

Much difference of opinion was expressed upon the fatality of this surgical procedure. While the statistics presented by the author of the operation indicated a mortality as high as 18 per cent., it was claimed by others that the mortality should not exceed two, or at most, three per cent.

The conclusion reached by a majority of the section undoubtedly was, that the sphere of oöphorectomy and the indications for the operation, were not yet definitely settled.

Great interest was manifested in the subject. The best men in the profession participated in the discussion, with an apparent candor and freedom from prejudice that did honor to the character of the profession and this important section of the congress.

The papers upon the various subjects were grouped, so that all upon the same topic were presented consecutively. Thus upon displacements of the uterus and their treatment, several essays were read. For the most part the authors assumed that the consequences were due mainly to mechanical disturbances, or at least they would justify this inference; for the one prominent idea appeared to be, that in the management of these cases, the mechan-

ical reposition and retention of the organ was the one great indication. Two criticisms might be advanced upon these productions, viz.: 1st. It is assuming too much to affirm that in cases where displacement of the uterus is detected, that the symptoms and sufferings of the patient are invariably due alone to the dislocation of the organ. It is in accordance with the experience of every practitioner, that displacements are sometimes detected unexpectedly, and have only an incidental connection with the ailment for which treatment is sought. 2d. While mechanical treatment may be required, this alone will fill but imperfectly the indications of the case. These patients are, as a rule, anæmic. The functions of excretion and elimination are imperfectly performed, digestion and assimilation are perverted, and neurasthenia results. The manifest indications are, while we restore the organ to its normal position, we should adopt measures to remove local congestion and inflammation by suitable topical applications. We should excite the secretions and eliminate the effete materials from the system. We should improve digestion and assimilation, increase the quantity of a good quality of blood, and thus improve the tone of the tissues, without which all treatment will be nugatory.

The papers, and the discussion upon the operation for the partial or complete extirpation of the uterus, seemed to possess a peculiar fascination, occupying much time and exciting great interest. The gentlemen who took part in the discussion, however, were not always those who had enjoyed the best opportunities for observation. The *nonchalance* of the speakers would lead one to suppose that the operation was but slightly, if at all graver, than the extirpation of the tonsils. Could the sex, who are not themselves responsible for possessing this much-offending organ, hear but a tithe of the talk of removing it, I am sure they would regret, to their latest hour, the fate that had made them subject to such mutilation. Carcinoma was the disease for which this operation was recommended. The last speaker in the discussion closed by affirming that however early, well and thoroughly the extirpation might be performed, the patient, though she survived the operation, would surely die of

the disease; to which sentiment there seemed to be a general assent.

One paper carried me back to the time of my earliest reading upon gynæcological subjects. It assumed that instead of laceration of the cervix uteri being the *fons et origo* of all uterine diseases, inflammation, with its consequences, ulceration, induration, *et omni genus* was the real pathology and the important condition to recognize, in the treatment of women, and that not a plastic operation, but local medication to remove these conditions, was the indication. Do the wheels of progress roll backward?

Two elaborate papers were presented upon post-partum hæmorrhage, which, after giving the causes and describing the conditions of this complication of labor, were devoted mainly to the management of this accident. The main reliance of one author in extreme cases (not new by the way) was the injection of a solution of perchloride of iron into the cavity of the uterus. This suggestion did not meet with the unchallenged approval of the section. In the discussion that followed, the great dangers liable to follow this mode of treatment were not ignored.

I was not a little surprised that the authors of these papers did not make more emphatic reference to two important means of management in sudden and great loss of blood after delivery, viz.: 1st. Direct pressure with the hand upon the site of the detached placenta, assisted by counter-pressure from without, sufficient to compress the uterus into a small compass, and down to the brim of the pelvis; and 2d, the importance of position, which can be utilized without interfering with other means in the least by raising the foot of the bed, so that, if need be, the body of the patient will form with the horizon an angle of 45°. This will secure the circulation of the little remaining blood in the system between the heart and brain, and thus bridge over the impending chasm of death, just ready to receive the patient.

But I would weary you, were I to follow this review further. It should not be inferred from the preceding, that no important or useful additions to the sum of our knowledge were made at the congress. I would by no means intimate that this was the case. On the contrary, it will only be necessary to remind you, gentlemen,

that many of the best-known members of the profession throughout the world were present,—physicians who had made for themselves a world-wide reputation, and that they contributed their profoundest thoughts and the results of their large experience, thus adding to the interest of the meetings, and giving instruction to all who had the good fortune to be present.

Of the comparative interest or value of the work done in the different sections into which the congress was divided, it would be impossible to speak from personal observation. That irrelevant and crude remarks were occasionally made in the discussions is undoubtedly true; but these were not of sufficient frequency to give character to the proceedings of the meetings.

The addresses delivered at the general meetings were, without exception, full freighted with profoundest thought and advanced ideas sufficient to satisfy the exactions of the most inquiring minds of the day. That by Virchow was, as a plea for vivisection, classic; and when generally circulated, as it certainly should be, will exert a strong influence upon the popular mind now being arrayed against this practice for physiological and scientific ends. The following brief extracts are from the address. The animus of the objections are like the following:

“In the name of humanity, of morality, of religion, the suppression of experiment on animals is demanded. * * * The criterion is pain. Everything by which, in the way of experiment, pain is inflicted on the animal, is torture of animals, and so far immoral and contrary to religion.” * * * The Professor answers: “The evidence that moral earnestness is failing in modern medical circles, is nowhere afforded. The reproach that Christianity is imperiled by vivisection, is worthy of Abdera. The assertion that the medical youth are inevitably “brutalized” by dissection and vivisection, is, as usual, snatched from the air; and it is also a calumny that vivisectioning teachers have suffered injury to their morality.”

That by Professor Pasteur, on the germ theory, excited great interest. It was from the experiments of Pasteur that Lister drew the inspiration which led to the valuable antiseptic method. And Dr. Davaine declares, “that his labors upon charbon (splenic fever or malignant pustule) had been suggested by Pasteur’s

studies on butyric fermentation, and the vibriion which is characteristic of it." In this address he gives the data of a further advance by utilizing these investigations, in the prevention of other transmissible diseases, promising to equal in importance that of vaccination as a preventive of variola.

The general address on surgery by Prof. Volkmann, brought to view in clear light the recent improvements in his specialty. His own words shall give you an idea of his mode of treating his subject. "To-day we may say, with the deepest conviction, that the surgeon is responsible for every disturbance that occurs in a wound; that it is his fault if even the slightest reaction or redness is developed in it, or if an amputation is not healed by first intention. He must reproach himself severely if, after an operation, bagging of pus occurs, and especially if death occurs from pyæmia. He who cannot attain to this degree of perfection, may be converted from his former method of treatment to antiseptic methods like one who, having hitherto always prescribed senega for catarrh, now uses ipecacuanha. Of the storm that has swept over the fields of surgery during the past ten years he has not experienced a breath."

The masterly production of Huxley met to the fullest extent the highest expectations. In his sweeping survey of the progress of medicine from the earliest times to the present, it was possible to see, as with the natural eye, the dawn brightening into the effulgence of mid-day. Like some ship in mid-ocean surrounded by dense fogs which obscure the vision, so that the firmament, the horizon, and all objects, though near, are shut out from view, till the rays of the rising sun penetrate and dissipate the clouds, and render the atmosphere translucent, when every object is presented in distinct outline upon the "deep blue sea" even to the distant blending with the sky,—so the mists of ignorance and uncertainty, which have enveloped medicine and the race, have been gradually dissipated by the accumulated light of experiment and observation of the centuries. The Professor expressed the strongest confidence that medicine would ere long occupy a place with the exact sciences.

The social features of the congress added in no small degree to the interest of the occasion. The hospitality extended to the

delegates, both public and private, was on a scale of magnificence which it is difficult to imagine could be excelled. The cordiality of the reception, and the personal attentions, were so spontaneous and constant as to excite the highest gratification in every stranger in attendance upon the congress. Though taxed to a great degree by the unprecedented attendance, nevertheless English hospitality was fully equal to the emergency.

I now pass to other topics of much interest to us as obstetricians. The study and the practice of obstetrics and gynæcology, at first view, appear to be distinct and independent departments of medicine. The difference, however, which in outward aspect are so marked, are in many respects apparent only. An interdependence and close relation exist in many, possibly in a majority, of the important cases which come under treatment. The complications and accidents of parturition, and the sequelæ of abortion, entail upon the sex many pathological conditions which are permanent, and cause great suffering and derangement of function. Nor is it stating the case too strongly to assert that these complications are for the most part avoidable. The inference that the accoucheur is largely responsible for much of the practice of the gynæcologist, is one that I do not propose to defend or deny.

Every obstetrician must have seen some cases, it may be not a few, in which serious consequences have resulted from injudicious interference, or from the neglect to render timely assistance. Unfortunately "meddlesome midwifery" is not always confined to the practice of the uneducated midwife. Like errors will be followed by similar results, though committed by the stronger sex.

I feel justified in presenting some suggestions upon topics, which for a long time I have considered important, viz.: How to prevent some of the accidents of labor.

Of course but little more than general statement is all that the time at my disposal will permit. Nor do I deem it expedient in this presence to elaborate in detail the propositions which I may advance.

At the present time great importance is attached to laceration of the cervix uteri. I do not introduce the subject on this occa-

sion to discuss the pathological significance of this accident. Undoubtedly, however, minor cases of laceration have, not infrequently, been elevated into undue importance, and symptoms have been attributed to these slight cases which the history would not justify. Unfortunately this, like other important novel advances in medicine, has been carried to an extreme, which time, a more mature reflection, and careful observation will correct. My object is to consider some of the preventable causes of laceration of the cervix uteri.

This accident may result from the application of an undue amount of force, either natural or artificial, to the cervix, while the tissue of the os uteri is in a condition unfavorable for rapid dilatation. Who can doubt that the increasing frequency in the application of the obstetric forceps within the os uteri, is a cause of laceration of the cervix? To my mind, this practice, as a cause of the accident of which I am now speaking, is so undeniable as to require no argument to convince any. Demonstration could be adduced, not from experiment upon the cadaver only, but from observation of the effects upon the patient. I protest with emphasis against such use of the forceps, which has under the teaching of some, otherwise respectable authorities, become too general and too indiscriminate. Something should be left to the natural forces, in so important a function as parturition.

The undilatable os uteri may predispose to laceration of the cervix. Different conditions of the os may be met with, any one of which may constitute an important factor in retarding the process of dilatation; as irritability caused by the premature escape of the liquor amnii, rigidity from pre-existing disease, as chronic inflammation or hyperplasia. In such cases the contractions are liable to be violent. The indications to soften the tissue of the cervix, and to diminish the force of the contractions, can be fulfilled by applying emollients locally, and by the administration of sedatives, *veratrum viride*, *morphia*, *chloroform*, *chloral*, etc. By these means I am confident the dilatation has been facilitated, and laceration prevented. Again, not so infrequently as to be remarkable, have I met with cases of delay in the first stage of labor, from a cause which I suppose depended upon undue

attachment of the membranes to the cervix uteri. In a model labor, the maturity of the ovum will be evidenced by a loosening of the membranes; the contractions of the uterus draw the os and inferior portion of the cervix away from the inferior segment of the membranes, while at the same time the membranes, with the liquor amnii, protrude through the os. By these means the os yields and dilates. If, however, the attachment of the membranes is too strong for this easy separation, labor is delayed, rupture of the membranes takes place before the os is dilated, and finally laceration takes place.

When delay in dilatation is met with, the tissue of the os and cervix being normal, the separation of the membranes from the cervix by the finger will be followed by the natural progress of the labor. I join issue with the assumption that the cervix is necessarily ruptured in labor.

Vesico-vaginal fistula may undoubtedly result from the unskillful use of instruments. More frequently, I believe, this accident should be attributed to some mismanagement of labor, or neglect of the patient after delivery. During a prolonged second stage of labor, the bladder should be emptied by the catheter, if necessary, at short intervals. After delivery, in severe labor, neglect to introduce the catheter at proper intervals will subject the weakened coats of the bladder to distension, and laceration into the vagina may take place. When any suspicion exists that these tissues have been injured by severe or long continued pressure, the catheter should be introduced at short intervals, not so much for the relief of suffering, as to prevent the laceration that would be liable to take place in the contused and weakened tissue of the bladder and vagina if distension should be allowed.

It will occur to every gentleman present, that great delay in the second stage of labor should not be permitted. Skillful delivery would certainly protect many patients from the distressing accident under consideration.

Rupture of the perinæum may result, like laceration of the cervix, from sudden distension of the tissues involved. A rapid termination of the stage of expulsion is far from being desirable. Time should be allowed for the tissues to yield. The length of time required must be determined by the attendant in any given

case. But how gain time when the contractions are too violent for the safety of the perinæum? The effect of violent expulsive force may be lessened by directing the patient to cry out at the commencement of each contraction, and the advance of the presenting part should be retarded, as it can be by direct pressure. But what I believe to be more important in preventing rupture of the perinæum, is the judicious management of the head at the time of its expulsion, and this is accomplished when *extension* of the head is prevented, till the occiput has escaped in part beyond the arch of the pubis. If extension is allowed to take place too early, the effect would be to bring the largest diameter, viz., the occipito-mental, in relation with the vulva, so that a great increase of dilatation would be required in the escape of the head.

One so frequently hears practitioners recount the difficulties experienced in effecting the delivery of the placenta, that we would almost be justified in concluding that in this act nature is at fault, and that here we meet with a departure from the law that governs every other part of the great function of reproduction. Everywhere else we see in this function the most perfect and harmonious adaptation of means to an end, the parallel with which we will search for in vain elsewhere. Or may we not say rather, that the phenomena of expulsion of the placenta may not have been fully comprehended. It does not appear reasonable to suppose that, in this function, inherent difficulties should be met with often, so serious as to thwart the process of nature.

The ordinary delivery of the placenta is a very simple duty. At the same time, the proper delivery of the placenta is a very important duty. Nature requires but little assistance usually, but that little assistance should be given at the right time and in the proper manner. It is a serious error to forcibly extract the placenta too soon after the birth of the child. This practice causes hæmorrhage, syncope, and endangers inversion of the uterus. In natural labor, I would insist upon the necessity of permitting the contractions of the uterus to separate the placenta and close the sinuses. The time required for this would vary from fifteen minutes to half an hour. So much depends upon the manner of the delivery of the child, that it is impossible to do more than to approximate the time that would be required by

nature to separate the placenta. Manifestly, the uterus would contract more slowly, and there would be a corresponding liability to hæmorrhage, were the child delivered suddenly, than if the inferior extremities were allowed to remain till uterine contractions expelled them.

Not only will the placenta be managed in this manner most successfully, but the patient will thus be best protected from post-partum hæmorrhage and septicæmia as well.

This word septicæmia leads me to add one other suggestion, and that refers to the necessity for the most careful cleanliness on the part of the accoucheur during the labor, and of the patient after delivery. There are other topics which I would be glad to present, but I have trespassed upon your patience too long already.

Of the work performed by this society during the year, it can hardly be necessary for me to speak except in general terms. The regular number of meetings have, I believe, been held, and I am happy to add that the members have faithfully responded to the requirements of our compact, in presenting papers and conducting discussions. The amount of work done by the society, as well as its quality, can best be appreciated by those who have been present at the meetings. The number of papers which have been printed has not been large, nor has the publication of the discussions been extended, and therefore it would be impossible from this source to form a just estimate of the quality of the work done or of the efficiency of the society.

I would do violence to my own feelings, did I neglect to thank the officers of the society for the punctuality and regularity with which they have, individually and collectively, discharged their several duties, and to each of the Fellows I extend my heartfelt thanks for their uniform courtesy during the year.

ARTICLE II.

A CONTRIBUTION TO THE MINUTE ANATOMY OF THE SKIN.

BY C. HEITZMANN, M.D., of New York. Read before the American Dermatological Association, Newport, R. I., Sept. 1, 1881.

The apparently complicated structure of the integument becomes easily understood, if we keep in mind that there are but two main tissues entering the structure of the skin, viz., connective tissue and epithelium. The connective tissue produces the flat layer, called derma; the epithelium covers the derma on its outer surface. The boundary line between the two formations is not even, but fluted, supplied with numerous small protrusions of the derma, the so-called papillæ, the sum total of which bears the name "papillary layer." The bundles of the connective tissue everywhere run an oblique course; they are arranged in the shape of a coarse reticulum in the lowest portion of the derma, whose rhomboidal meshes contain a varying amount of fat-globules, the so-called subcutaneous tissue. In the derma proper, the bundles run in two almost rectangularly interlaced directions, thus producing a very dense felt, which by being tanned gives the leather. On the lowest portions of the derma the bundles are relatively coarse; they become the finer the nearer the papillary layer, and in the latter very delicate connective tissue fibers are noticeable only without a distinct arrangement into bundles. The epithelial formations on the top of the derma, again, exhibit two main layers, the lower one, that nearest to the papillary layer, being alive and supplied with nerves, the so-called rete mucosum; while the outermost layer is composed of dry, horny epithelia, giving the formation called epidermis.

The connective tissue is supplied with blood-vessels and lymphatics; the epithelium lacks such formations.

If we now imagine that the connective tissue, together with the covering epithelium, were a pliable sheet, for instance, of chamois, and we produce a depression of this sheet with one of our fingers—the result will be a pouch, whose innermost layers are epithelial, whose outermost layer is connective tissue. The epidermis will cover the inner surface of the pouch, and now bear

the name "inner root-sheath;" next to this will be a layer, formed by the epithelia of the rete mucosum, which will be the "outer root-sheath;" the outside of the pouch must be connective tissue, and will represent the "follicle." On the bottom of the pouch will be a protrusion of the follicle, kindred to those on the surface of the skin, therefore connective tissue, the "papilla of the hair."

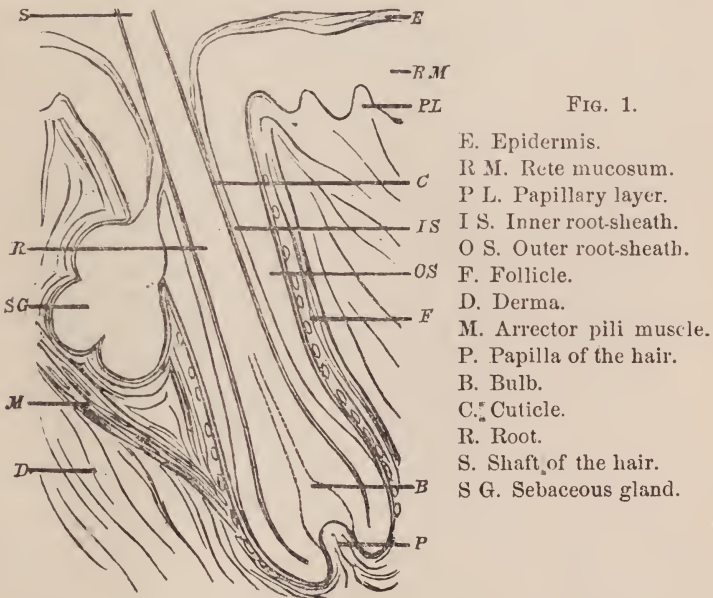
On our diagram slight alterations must be made. The epidermis, which is composed of a large number of flat epithelia, greatly varying in accordance with the width of this layer, upon entering the pouch and becoming the inner root-sheath, will gradually be reduced to a limited number of horny epithelia, in the midst of the pouch to not more than two strata. Near the bottom of the pouch the number of the epithelia again increases, the inner root-sheath gains in width, and is composed of three or four strata of epithelia, which have lost their horny character, and become of a protoplasmic nature again. The rete mucosum enters the pouch in its full width, but gradually becomes thinner, namely, composed of a smaller number of epithelia, which retain their original protoplasmic character, and at last, near the bottom of the pouch, after being reduced to a single layer, completely disappear.

Imagine, now, that against the bottom of the epithelial pouch, which, according to the main direction of the connective tissue bundles, runs in an oblique direction, a pin is pressed and the pouch turned upward again. This procedure, of course, will involve the inner root-sheath exclusively, and an elongation must result, of an epidermal character, according to the main character of the inner root-sheath. This elongation represents the hair.

The hair, therefore, is a solid elongation of the hollow inner root-sheath, and produced by the inner root-sheath alone. The outer root-sheath has nothing to do with the formation of the hair. On the bottom of the pouch there is a knob, composed of living protoplasmic epithelia, like those of the inner root-sheath in the same situation. This knob is called the bulb of the root of the hair, and directly surmounts the papilla of the hair. Higher up the epithelia become horny once more, and go to build up the root and the shaft of the hair.

Imagine, lastly, that on the side of the acute angle of the obliquely implanted pouch, the outer root-sheath, as said before, an off-shoot of the rete mucosum, be pushed laterally and downward by a pin, the result will be a third elongation, produced by the outer root-sheath, a small pouch itself, bearing the name "sebaceous gland." According to this diagram, the sebaceous gland is exclusively a formation of the outer root-sheath, while the inner root-sheath has nothing to do with the formation of the gland.

The idea of the production of the root-sheaths, the hair and the sebaceous glands, is illustrated by the accompanying diagrammatic figure 1. The explanation is as follows:



The epidermis, bulging downward, results in the formation of the inner root-sheath, while the rete mucosum, elongated downward, results in the formation of the outer root-sheath.

The bundles of the connective tissue of the derma, which give an outer investment to the pouch, composed of both root-sheaths, produce the follicle. Its innermost portion exhibits cross-sections of smooth muscle-fibers.

The papilla of the hair is a product of the follicle. Around the papilla is a knob, the bulb of the root of the hair, which continues into the root of the hair—that part inclosed in the pouch; and the shaft of the hair—that part standing forth on the surface of the skin.

The diagram shows that the inner root-sheath, upon approaching the bottom of the pouch, becomes widened, and on the bottom of the pouch turns over, thus first producing the bulb, afterward the root, and the shaft of the hair itself. The innermost layer of the inner root-sheath, by turning over, results in the formation of the cuticle, the single investing layer of both the root and the shaft of the hair.

The figure demonstrates, furthermore, that the outer root-sheath, upon approaching the bottom of the pouch, grows thinner and perishes at last, while on one side the outer root-sheath produces the pouch of the sebaceous gland. Between the outer root-sheath and the follicle there is a homogeneous layer, the so-called “structureless membrane.” The arrector pili muscle is in connection with the muscle-layer of the follicle, and surrounds the bottom of the sebaceous gland.

The diagram serves as a key, which enables us to easily comprehend all formations of the skin engaged in the formation of the hair. First, let us study the upper portion of the hair-pouch from a specimen of the human skin, fig. 2. The pouch, as a rule, has a funnel-shaped widening on the surface of the skin, which is covered by stratified epidermal scales. These scales are traceable in direct union to the inner root-sheath, which begins on the so-called neck of the pouch, being composed of two epidermal layers only, and in honor of the discoverer, termed *Henle's* sheath. Next to the inner root-sheath lies the extremely delicate cuticle of the hair, which ensheaths both the root and the shaft of the hair. With higher powers we see on each hair finely serrated edges, the slightly bulging edges of the cuticle. The hair is composed of closely packed, horny epidermal spindles, which hold a varying amount of pigment granules. The rete mucosum directly elongates into the outer root-sheath, and this into the sebaceous gland. It is only the duct of this gland which is covered by flat horny epithelia, while the gland, as such, is

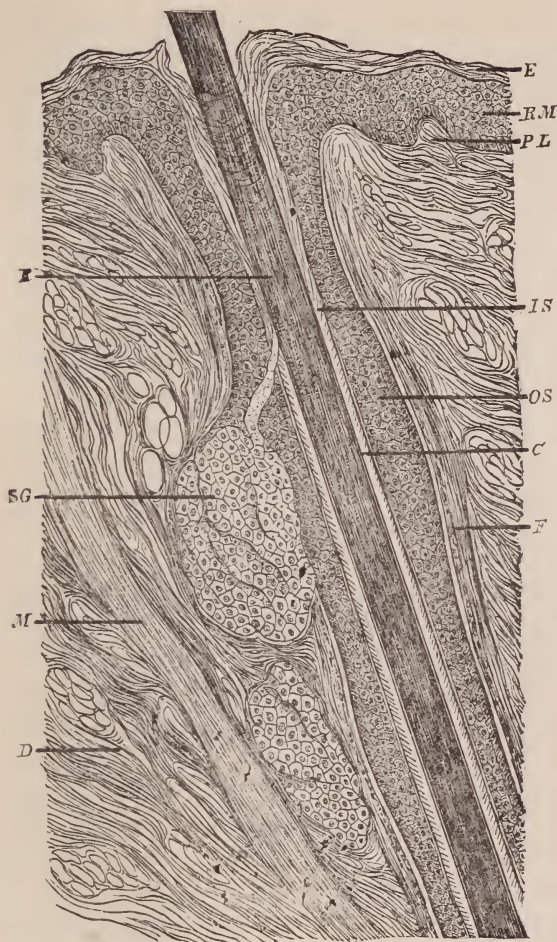


FIG. 2.

- E. Epidermis.
 R M. Rete mucosum.
 P L. Papillary layer.
 D. Derma.
 F. Follicle.
 M. Arrector pili muscle.
 S G. Sebaceous gland
 O S. Outer root-sheath.
 I S. Inner root-sheath.
 C. Cuticle.
 R. Root of hair.
 Magnified 150 diam.

composed of cuboidal epithelia, like any acinous gland. The duct of the sebaceous gland, as a rule, empties into the funnel-shaped widening of the pouch, in the space between the inner root-sheath and the hair, or, more particularly, its covering cuticle. The outer root-sheath is composed of several strata of epithelia, like the rete mucosum itself. The strata are cuboidal epithelia, and it is only the stratum next to the derma, or, more particularly, the structureless membrane, which is formed by columnar epithelium. The inner surface of the structureless membrane, again, is covered by extremely delicate, flat endothelia, as first demonstrated,

with the aid of silver-tinction, by *Czerny*. These endothelia, by very marked prickles and thorns, are directly connected with the adjacent columnar epithelia. To the presence of these thorns *D. Haight* first drew attention.

The pouch of the sebaceous gland is under the control of the arrector pili muscle, which represents a flat fan-like sheet, whose broad ends terminate in the papillary layer, while the narrow end is inserted in the follicle of the root of the hair. No doubt, the evacuation of the sebaceous gland is done by contraction of this muscle-sheet. The fatty mass will be squeezed first into the funnel of the hair-pouch, as a rule, and only on large sebaceous glands directly to the surface.

The lower extremity of the hair-pouch, in specimens taken from the human skin, is not readily comprehensible, unless upon the basis of the study of hairs of animals, especially of those strong hairs on the upper lip of kittens. This, perhaps, is the reason why, after many years' lively writing, not one author has given a plain description of the relations. As a matter of course, the essentials are identical in the hairs of kittens and those of man, though the former are, as a rule, more plain than the latter.

Fig. 3 illustrates the bottom of a hair-pouch from the lip of a kitten.

The inner root-sheath in its upper portion shows the light, horny *Henle's* layer. In an oblique line there appear polyhedral epithelia; at first pale and finely granular, with indistinct nuclei; deeper down coarsely granular and slightly elongated. This latter portion of the inner root-sheath represents what has been termed *Huxley's* layer. It is seen that on the bottom of the pouch this layer turns over, surrounds the papilla and constitutes the bulb of the root of the hair. The epithelia on the lower periphery of the papilla are columnar, gradually changing into the cuboidal form, and more upward becoming elongated, spindle-shaped. Lastly, they emerge into the horny spindles which produce the main bulk of the hair. The boundary line between the inner root-sheath and the root of the hair is given by a thin, apparently structureless layer, outside of which is the inner root-sheath, inside the cuticle of the hair. The cuticle on the upper

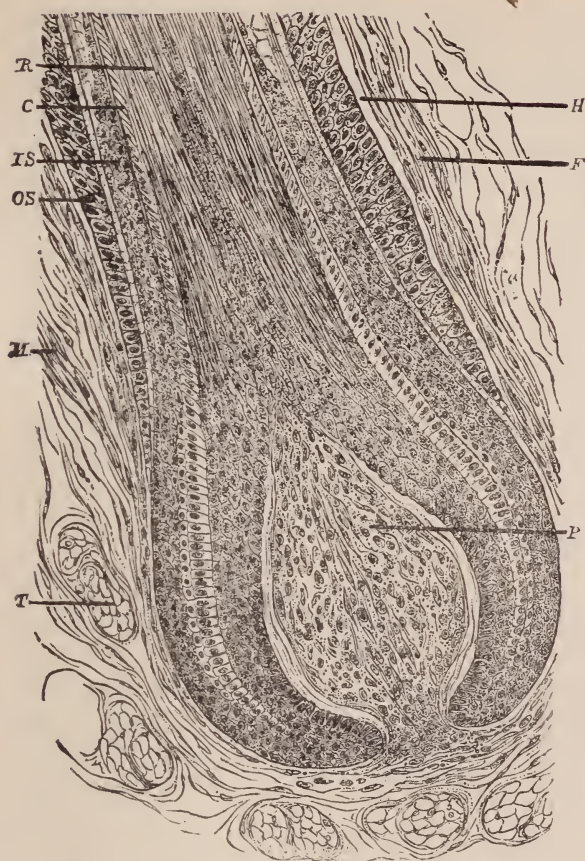


FIG. 3.

- F. Follicle.
 T. Transverse sections of connective tissue bundles of derma.
 M. Arrector pili muscle.
 I S. Inner root sheath.
 O S. Outer root-sheath.
 P. Papilla.
 C. Cuticle.
 R. Root of hair.
 H. Hyaline, or so called structureless membrane.
 Magnified 300 dia

portion of the root is composed, as well as on the shaft, of thin, imbricated scales, whose edges are slightly bulging forth over the surface of the hair, and give the latter the peculiar serrated appearance. Gradually the epithelia of the cuticle of the root assume a columnar shape and become nucleated. At the height of the bulb these columnar epithelia are very large, paler, granular, supplied with large and distinct nuclei. Their characteristic row runs in the middle between *Huxley's* layer and the bulb, at last blending with the cuboidal epithelia of both formations. Outside of the cuticular row there is another thin layer of flattened, pale epithelia, which evidently corresponds to the innermost structureless layer of the inner root-sheath. The middle portion of the

bulb is often occupied by globular, indifferent or medullary corpuscles, which hold a varying amount of pigment, and fill also the central portion of the root, the so-called medullary space, which even in strong hairs may be absent. The outer root-sheath is composed, in its upper portions, of stratified epithelia, the outermost layer being distinctly columnar. The latter row is the last one left, as the outer root-sheath approaches the region of the bulb, and gradually becoming thinner, in turn is entirely lost at the height of the bulb, whose formation it does not enter at all. The boundary line between the outer and the inner root-sheath is again marked by the presence of a so called structureless membrane. Outside the outer root-sheath we find the follicle, a connective tissue formation, with interspersed circular muscle-spindles, in connection with those of the arrector pili muscle. Between the follicle and the outer root-sheath there is usually a broad homogeneous layer, which can be traced around the bulb of the root and the papilla of the hair.

The papilla of the hair is composed of a delicate fibrous or myxomatous connective tissue, freely supplied with protoplasmic bodies and spindle-shaped nuclei, and traversed by a number of capillary blood-vessels. The apex of the papilla in our specimen is not distinctly separated from the epithelia of the hair. The line of demarcation, however, as a rule, is distinguished by the presence of a row of columnar epithelia, or the medullary corpuscles.

Outside of the follicle there is the fibrous connective tissue of the derma, built up by longitudinal and transverse bundles.

In comparing what I have said about the theory of the formation of the hair with specimens of the skin, a satisfactory congruence will be found. This theory, as I have taught it for nearly seven years in my laboratory, will explain the fact that the inner root-sheath is withdrawn simultaneously with the root upon pulling a hair. It furthermore explains the process of shedding and the new formation of the hair.

We know through *Kölliker* and *C. Langer* that the young hair is formed around the old papilla. We know that at a certain height above the papilla there is a knob-like thickening (*Henle*), which corresponds to the bulb of the falling hair. The fact that

I add is that the new growth of a hair takes place within the realm of the inner root-sheath exclusively.

Fig. 4 is a diagram of the process of shedding. The inner

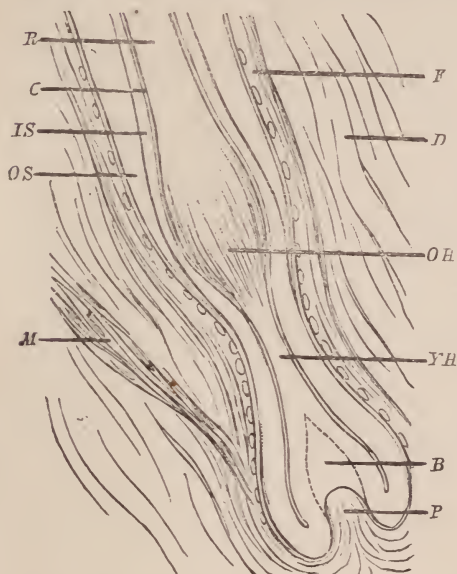


FIG. 4.

- F. Follicle.
- Arrector pili muscle.
- P. Papilla of hair.
- B. Bulb of hair.
- I S. Inner root-sheath.
- O. S. Outer root-sheath.
- C. Cuticle.
- R. Root.
- O H. Old hair still in connection with
- Y H. the young hair growing from bulb.

root-sheath, below the bulb of the old hair, which latter is fringed by the torn epidermal scales, gradually becomes widened. On the bottom of the pouch it turns over and produces the bulb, which is composed of medullary, or indifferent or embryonal corpuscles. The boundary between the two portions of the inner root-sheath is established by the cuticle, which, below the bulb of the old hair, is composed of columnar epithelia. The pigment, where there is any, lies exclusively in the central portion of the inner root-sheath, the future hair. The outer root-sheath has nothing to do with the new formation of the hair. The smooth muscles of the follicle evidently play a part in the process, inasmuch as through their contraction a narrow neck is established around the young hair, as first suggested by *Biesiadecki*.

With the views here advocated, also, the formation of the sudoriparous glands becomes understood. We simply may assume an elongation of the outer epithelial layers into the depth of the derma. The duct within the epidermal stratum shows windings,

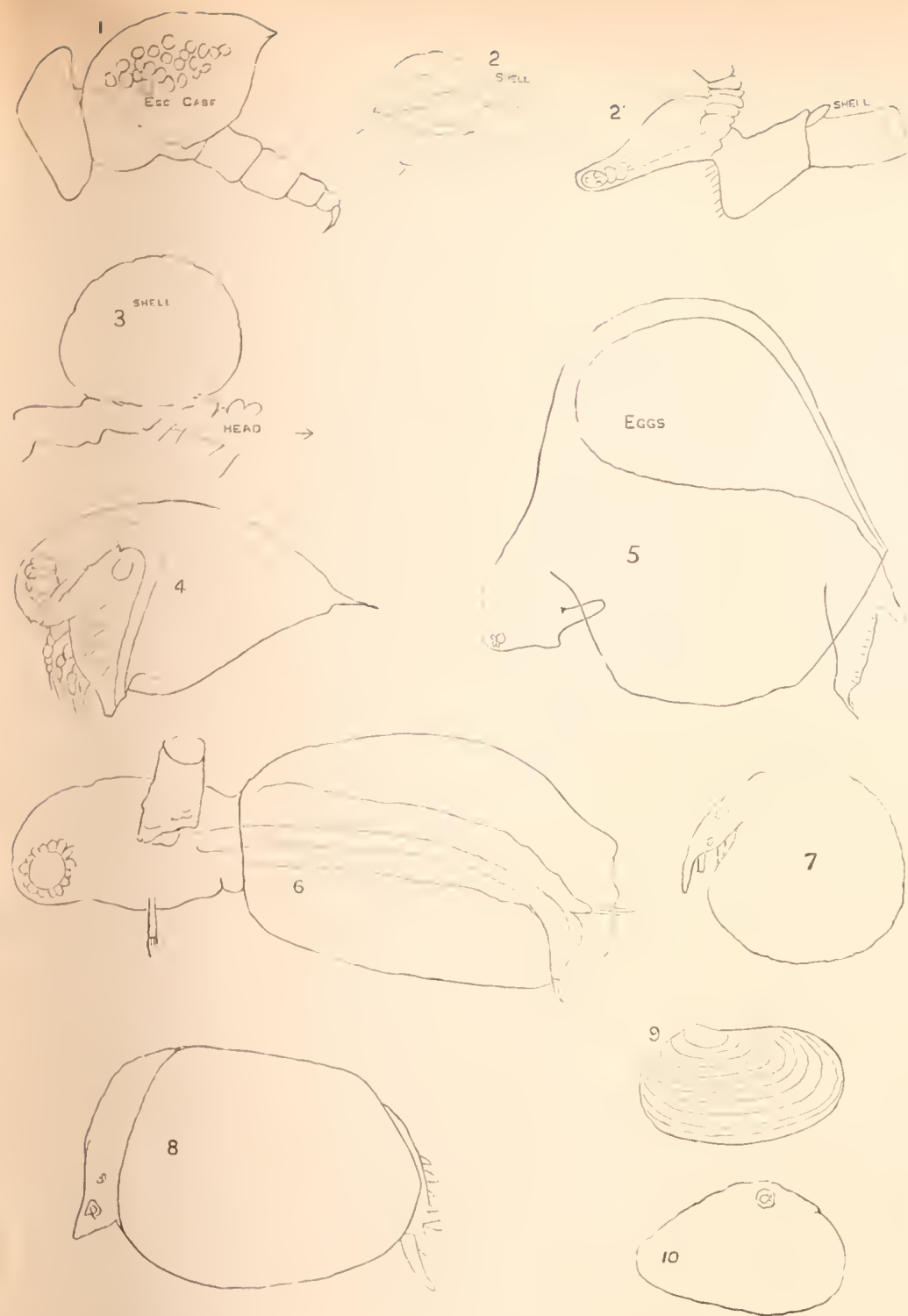
which, on places where the epidermis is thin, scarcely are perceptible, while on places, such as the soles of the feet, where the epidermis is very thick, they exhibit the characteristic corkscrew-like appearance. The duct in this situation is lined by flat epithelia. On the point where the duct pierces the rete mucosum, in the valley between two papillæ, as a matter of course, it must be lined by stratified epithelia. Within the derma the duct is composed only by one layer of columnar epithelia, which obviously are an elongation of those of the lowest layer of the rete mucosum. The tube, as it begins to coil up, is lined by cuboidal or short columnar epithelia, also in one layer. Both the duct and the coil are invested by a somewhat denser connective tissue, and supplied with blood-vessels. Between the connective tissue and the epithelium there is sometimes a distinct structureless or hyaline layer. Larger coils, especially those of the axillary pit, we know to be supplied with smooth muscles, which are imbedded in the ensheathing connective tissue capsule.

ARTICLE III.

NOTES ON CRUSTACEA IN CHICAGO WATER SUPPLY, WITH
REMARKS ON THE FORMATION OF THE CARAPACE. By
E. A. BIRGE, Professor of Zoölogy, University of Wisconsin,
Madison, Wis.

Through the kindness of President B. W. Thomas, I have from time to time received valuable specimens of the animal and vegetable life of the Chicago water supply, a subject to which he has devoted much labor. In these specimens I have observed the following species of Cladocera, the group of Crustacea in which I am especially interested.

- Sididæ. { *Sida crystallina*—O. F. Müller.
 { *Latona setifera* (?)—O. F. Müller.
- Daphnidæ. { *Daphnia pulex*—De Geer.
 { “ *galeata* (junior) (?)
- Bosminidæ. — *Bosminia longirostris* (?)



EXPLANATION OF CUTS.—LUTONA.

1. Antenna.
2. Antennule.
3. Angle of Antennule to show sense hairs.
- 4, 5. Post Abdomen.
6. Hair of Post Abdomen.
7. Hair of Antenna.
8. Macula Nigra from below.



EXPLANATION OF CUTS.—ORIGIN OF CARAPACE.

- A. Napius, shell of Cyclops, from above.
- A'. Napius, shell of Limnetis, from above.
- B. Outline of Cyclops, to show Carapace.
1. Egg-case of Dorypygus.
2. Anterior portion of Leptodora.
3. Shells of Female Leptodora.
4. Shell of Polyphemus.
5. Shell of Evadne.
6. Shell of Holopedium.
7. Shell of Daphnella.
8. Shell of Chydorus.
9. Shell of Young Estheria.
10. Shell of Adult Estheria.
11. Shell of Ostracoda—Gypris.



Lynicidæ.	{	Camptocercus macrurus—O. F. M.
		Pleuroxus insculptus—Birge.
		“ denticulatus—Birge.
		Chydorus sphæricus—O. F. M.
		Alona (?) two species unidentified.

All these species, except the second, are abundantly distributed in this country and elsewhere. They are what might be expected in any water derived from lakes, and do not argue any special impurity in the water. Indeed, with the doubtful exception of *Alona*, they are all species preferring the clearer waters. *Alona* is a genus of very various habits, some species being decidedly limicolous, while others haunt weedy but clear water, and others still may be found in places quite free of weeds. The list is thus, on the whole, conspicuous for the absence of dirt-loving Cladocera. The absence of some well-known species may occasion some surprise. None of the very abundant forms are present except *Chydorus*. One would have looked for some *Simocephalus* or *Ceriodaphnia*, and no doubt these will be found in due time.

I have at hand no very full description of *Latona setifera*, O. F. M., with which to compare the specimens sent me from Chicago. The two descriptions which I have—Leydig's and P. E. Müller's—are not in complete accord. Still, I think that our specimens belong to that species. They had been mounted, and so had been flattened, and unfortunately, in every case were vertically compressed. So delicate were they that I could not get them flattened in the other direction, and must content myself with some notes and rude sketches of some of the details of structure.

The animal is everywhere exceedingly rare. It has been seen in Germany, in Sweden and in Denmark, in Europe, by Müller Sars and P. E. Müller, respectively, and in this country by Mr. Edward Burgess, of the Boston Society of Natural History, in Cochituate water, and now by Mr. Thomas in Chicago. Other observers have probably met with it, but I have no record of the fact.

The size of the specimens is from two to two and a half mm. in length. The lower edge of the valves is armed with numerous

long setæ, from which the name of the species is derived. Leydig (*Naturgeschichte der Daphniden*) says that the anterior and posterior edges only of the valves are thus armed, but as he had not seen the species himself, this may easily be an error. Between each of the large setæ are three or four very small spines. Fornices are present. The rostrum forms a broad plate projecting over the base of the rather small labrum. The antennules are long, and covered with sense-hairs to the end. I find no trace of the "setæ sensuales obtusæ" described by P. E. Müller (*Danmarks Cladocera*, Pl. VI, fig. 22), and figured by him on the inside of the antennule, near the base. (See my figs. 2 and 3). The antenna (fig. 1) afford one of the most striking peculiarities of the animal. They appear three-branched, but in reality have but two branches, like those of all Cladocera. The basal joint of the two-jointed branch is projected out between the two branches, so as to look like a third division. The joints of the three-jointed branch bear respectively 4-0-0 setæ; of the other branch 8-9 setæ respectively; spines 0, 1, 0 and 0, 0, and one on the basal joint. These setæ are plumose, especially the terminal joint (fig. 7).

Both maxillæ seem to be present. This fact was noted by Claus (*Zeitschrift für Wissenschaft. Zoölogie*, Vol. XXVII) in *Sida*, but I have been entirely unable to confirm his observations by my own on American specimens. Here, however, both seem to be functional.

As in *Sida*, there are six pairs of legs, and the post-abdomen closely resembles that of *Sida*. It has eight abdominal teeth in each row. The terminal claws are provided with two spines, and a row of very fine teeth on each edge (figs. 4, 5). The abdominal setæ are two-jointed, and the terminal joint bears long, fine hairs (fig. 6).

The macula nigra is double (fig. 8).

On the Origin of the Carapace in Crustacea.—In his *Anatomy of Invertebrated Animals* (p. 315), and again in his *Crayfish* (p. 449), Prof. Huxley brings forward—not for the first time—his theory of the decapod carapace. Briefly stated, it is this: "The carapace corresponds with the terga and tergal halves of the pleura of all the somites, which are thus reflected into it,

and these include, without exception, all from the last thoracic to the ophthalmic." The ordinary theory is well known. It is that of Milne Edwards, that the post-mandibular part of the carapace is a reflection of the tergum and pleura of the mandibular segment.

It may be questioned whether a form bearing only three segments can be the primitive form of crustacea. Whether the nauplius, however, is or is not such a form, makes little difference, since all groups of crustacea pass into the nauplius stage, and their carapace is derived in some way from the nauplius-shell. This nauplius-carapace, then, we may call the *primitive carapace* (figs. A, A¹). It is an oval, unsegmented shield, hardest on the back, where it serves both for protection and for attachment of the muscles which move the appendages. These are now three, antennule, antenna, mandible. This carapace may barely cover the back, as in *Peltogaster* and many other forms, or, as in *Limnetis*, it may extend broadly out on the sides.

In no adult form is the number of segments so small as three, and in all forms which pass through the nauplius stage after leaving the egg, more or fewer of the segments which appear as development goes on, are anchylosed to the three already present. The two maxillary segments are always thus united, and others may be, up to the number of eight. Thus a sort of carapace may be found covering the first five segments at least. This carapace may, when developed, take one of two general forms, which we may name respectively the *copepod-form* and the *phyllopod-form*.

If the development of a form like *Cyclops* among the Copepoda be watched, it will be seen that the shape of the body and the direction in which the appendages point is gradually altered, as well as the shape of the appendages and the number of segments. The body, at first relatively flat and thin, becomes horizontally compressed, and the appendages, from pointing outward, are directed downward. The carapace also undergoes modification. It is extended downward into pleura which protect the bases of the legs, and enlarged by the addition to it of new segments, in *Cyclops* three in addition to those already present. In other Copepoda more or fewer segments may be added, but in

all cases the result is the same—a firm anterior shield is formed by the coalescence of the skeletons of the anterior segments, and this is extended downward into pleura to protect the appendages (fig. B). This form is found not only in Copepoda, but in the anterior shield of Trilobita, in Pœcilopoda, in the heads of Amphipoda and Isopoda, and in Arachnida and Insects. This form of carapace plainly agrees with Prof. Huxley's definition.

Very different is the origin of the other form. The nauplius larva of *Limnetis* gives us a hint of its source. The phyllopod form of carapace arises from the backward extension of the nauplius-carapace, covering the segments which are developed behind the mandibles. Our Cladocera show it in a well developed form, a large bivalve shell, covering body and legs, but free from both (figs. 5, 6, 7). In other forms it is more extended, enveloping not only body, but head as well. Such is the case in the Ostracoda, in *Estheria*, *Limnetis* and allied forms (figs. 10, 9). In these cases it always, I think, arises from the mandibular segment, although such is not necessarily its origin.

Before passing to consider to which form the decapod-carapace is allied, let us cast a glance at the possible origin of this form of carapace. In *Notopterophorus*, a copepod, we find the dorsal parts of the segments prolonged into a sort of crest, and in *Doropygus* we find the last thoracic segment expanded, and used as an egg-case (fig. 1). This case is of course homologous in a broad way to the two cases in which *Cyclops* carries its eggs, but here we have the skeleton more obviously made use of as a case.

Now, when we come to the Cladocera, we find the bivalve shell made use of as an egg-case, and in certain forms—the *Gymnomera* of Sars, including the genera *Leptodora*, *Polyphemus* and *Bythotrephes*—as such a case only. In these the carapace is rudimentary (figs. 2, 2¹, 3). *Leptodora* has a long segmented abdomen; only the females have a small shell, capable of holding two eggs; the males and young females are shell-less. *Polyphemus* has a small but permanent shell. *Evadne* has a much larger carapace (fig. 4), but one from which the legs are still free. *Holopedium* (fig. 5) has the regular phyllopod form, but has the egg-case still as a prominent feature. In *Daphnella* (fig. 6), we get the ordinary form.

In Chydorus we find the carapace beginning to encroach on the head, and this feature carried to extremes in Ostracoda and Limnadiæ (figs. 7, 8, 9, 10).

Now, is this series, from Daphnella to Leptodora, given in the direct or inverse order? Have we here a direct or a retrograde metamorphosis? Is the carapace a developed egg-case, or the egg-case a rudimentary carapace? The question is too large to be answered fully at present, but one or two considerations may be urged to show that the carapace develops from the egg-case. The Cladocera are a very old group, showing all sorts of cross-affinities and retention of primitive features where they would least be expected. Such is the curious modification of the primitive antennule—undoubtedly like that of Latona—seen in the Bosminidæ and in the males of Ceriodaphnia, and other such features might be adduced. Two groups are remarkable for retaining primitive structures—the Sididæ and Gymnomera—and the position turns on the decision as to which of them most nearly represents the primitive form. From the structure of the antennules, of the antennæ especially, and of the legs, I conclude that the latter represent more nearly the “Ur-cladocera,” and this conclusion is strengthened by the fact that Leptodora alone of the Cladocera has a free nauplius form. I believe that the phyllopod carapace originated as an egg-case.

To which form, then, is the decapod carapace allied? Whatever be the answer for Astacus, it seems tolerably plain for the rest of the Podophthalmata. Passing over Schizopods, a glance at the development of Peneus will show that its carapace belongs to the phyllopod type. It is free from the body behind the mandibles, and remains so for a very considerable time. So, too, the carapace of Squilla remains permanently free. The same is true for all decapods which pass through the zoëa stage. The zoëa-carapace is plainly of the phyllopod type (to which group, indeed, the zoëa is allied in many ways), and the carapace of the adult is derived from that, either directly, or indirectly, by means of the megalops stage. In no case of decapods which undergo a metamorphosis, can the carapace be referred to the copepod type.

What modifications this carapace may undergo in so modified a

development as that of *Astacus*, may be a subject for further discussion. But plainly the inference which follows from Prof. Huxley's treatment of the subject in his *Invertebrates*, is incorrect. The decapod carapace in general conforms to the phyllopod type, not to that of the Copepoda.

I do not think Prof. Huxley's position tenable for the crayfish itself. On this point I speak with great diffidence, since I have never investigated the embryological development of the crayfish for myself. In studying that of *Panopæus*, I observed that the carapace, soon after its first appearance, apparently belonged to the segments as far back as the first maxillipedes. Subsequent changes showed that this was not the case, but that the still relatively undifferentiated condition of the blastoderm gave rise to the appearance. After hatching, the true relations of the carapace plainly appeared. In the passage from the zoëa to the megalops stage, the carapace, once free, became attached to the hinder thoracic segments. Now, my own belief is that in the crayfish, since it leaves the egg in a post-megalops stage, has syncopated this process of development and subsequent adhesion, and that consequently the appearance is that the carapace belongs to fourteen segments, instead of three. I have, however, no direct proof of this theory. It is only a "guess at truth," and I offer it merely as such.

ARTICLE IV.

STUDIES IN THE MEDICAL BOTANY OF SOUTHERN ILLINOIS.

By J. M. G. CARTER, A.M., M.D.

II.

The therapeutic agents mentioned in this article all belong to the botanical division called Monopetalous Exogenous Plants. The division is large, and it contains a long list of valuable remedies, many of which are found in this section of Illinois.

Several plants of the Honeysuckle Family (Caprifoliaceæ) are used in practice. *Lonicera Caprifolium*, containing the species *L. Sempervirens*, or Trumpet Honeysuckle, and *L. Grata*, or

Woodbine, has been prescribed, in the form of syrups, for asthma and other lung troubles. In domestic practice the juice of the plant is frequently used for the bites and stings of insects. The bruised plant may be placed on the injury as a poultice, or the juice rubbed on as a linament. The fruit of all species of *Lonicera* is said to be emetic and cathartic.

To this order belongs Fever-wort, Fever-root, or Wild Ipecac (*Triosteum Perfoliatum*, *T. Augustifolium*). It is also called Horse Gentian. It is a bitter tonic in small doses. In larger doses it is a diuretic; and in doses of 20 to 30 grains of the bark of the root, it is emetic and cathartic.

Sambucus Canadensis (Common Elder) has been used in domestic practice as a tonic in the form of "bitters." This member of the *Caprifoliaceæ* is tonic, diaphoretic, diuretic, alterative, emetic and cathartic. The juice of the berries is aperient, and has been recommended in constitutional diseases and eruptive affections as an alterative. It has been thought of especial value in rheumatic, syphilitic, and gouty diatheses. For these diseases the juice of the root or berries may be given in doses of fifteen drops four times a day. In tablespoonful doses it may be used as an emetic; or as a hydragogue cathartic in dropsy. One dose a day will probably be sufficient for either purpose. It is sometimes recommended for epilepsy. How does it act in this malady? This is a question which I cannot answer. I have not used the remedy in this disease; but I presume that whatever benefit is derived from it must be due to the valerianic acid and tannic acid contained in it. If epilepsy is due to variations in the caliber of the cerebral vessels, as has been asserted, the sedative and astringent properties of these constituents might have some slight beneficial effect.

Perhaps the most important genera of *Caprifoliaceæ*, considered from a medical point of view, is *Viburnum*. The particular plant of this genus to which I wish to call attention is *V. Prunifolium* (Black Haw.) This is one of the new remedies in regular practice, but the Eclectics have used it for many years. Its value as a tonic and uterine sedative has been brought before the profession by Drs. D. L. Phares, E. W. Jenks, and others. It is of especial value in threatened abortions. It may be given alone

in such cases, or used in conjunction with opium. It has been recommended as antispasmodic, astringent and diuretic.

I have used this remedy in about one hundred cases of uterine or ovarian diseases, and its administration has been attended with remarkable success. I have used it in ovarian neuralgia; neuralgia of the os uteri; endometritis (especially chronic), whether general, corporeal or cervical; areolar hyperplasia; dysmenorrhœa; amenorrhœa; and hysteria; and in all forms of neuralgia and headache supposed to be due to some uterine trouble. That is, I have used it as a uterine sedative to control pains and soreness in the region of the uterus, to control leucorrhœa, pains in the back, dragging pains in front, etc.

I used it in one case of neuralgia and leucorrhœa due to lacerated cervix uteri. I prescribed the fluid extract in 5ss doses four times a day. After a month the patient stopped taking the remedy, and her improvement, which had been satisfactory, has not progressed any since; still, her present condition is better than before taking the medicine.

The value of the remedy in dysmenorrhœa may be shown by referring to the case of a young married lady (married one year) who had been suffering every month for some two years, her trouble gradually growing worse. When I was called she was in one of her severest paroxysms of pain. I gave anodynes for temporary relief, and kept her upon a mixture of *V. Prunifolium* and *Valerian*, chiefly the former, until the next monthly sickness. This time her suffering was so slight that I was not consulted. She continued taking the mixture, having no special pain or sickness next month, and has not had since—several months. A case of hysterical paroxysms, which had been increasing in frequency and severity for four years, but which yielded to the action of the Black Haw in less than three months, might be described.

Eupatorium Perfoliatum and several other varieties of *Eupatorium* (*E. Altissimum*, *E. Sessilifolium*, *E. Scrobinum*), are found in S. Illinois. *E. Perfoliatum* is generally called Thoroughwort in the North and East, but in this section it is better known as Boneset. Boneset is one of the best-known remedies in our portion of the State. Many persons make free use of it when feeling

“bilious,” dull headache, impaired appetite and general malaise. I have frequently prescribed it in such cases myself with excellent results. Sometimes I direct a tea to be made of the leaves or tops, to be taken *ad libitum*. Frequently I direct two ounces of the leaves to be boiled in a pint of water, and prescribe one or two tablespoonfuls t. d., generally before meals.

When it is desired to keep the preparation for some time, it is better to add an ounce of alcohol to each pint. It is said that the cold infusion, or the powder, is best for its tonic effects. The infusion should be warm, and the patient should be covered up in bed to obtain its diaphoretic effect. The decoction I have mentioned may be given in doses of ℥iv to ℥viij for the action of the remedy as an emetic or cathartic. It is used in intermittent, remittent and typhoid fevers.

Erigeron belongs to the same family as Eupatorium, Compositæ. The Erigeron Canadense is the most important variety or species found in Southern Illinois. I believe E. Philadelphicum and E. Annuum are found in this part of the State also, but I am not aware of any general or domestic use of them other than what is given in the books.

Golden-rod (Solidago) is another Composite of medical interest which grows abundantly in this region. The varieties found here are S. Bicolor, S. Speciosa, S. Petiolaris, S. Missouriensis, and others. This aromatic, stimulant, carminative, and diaphoretic, is not used by the common people and rarely by the profession, so far as I am aware. Ambrosia (Ragweed), two or three species of which are found, is an Eclectic remedy—a tonic in low fevers.

Helianthus (Sunflower), another of the Compositæ, I mention only because it has the reputation of protecting against malaria. It is said that malarious districts have been rendered healthy by planting H. Annuus. The pith of the same plant has been recommended for the preparation of moxa on account of the niter which it contains. To the same order belongs the Bidens Bipinnata (Spanish Needles.) This has some reputation among the people as an emmenagogue. The root and seeds of this and other species (B. Froudosa, B. Connata) are thus used, and are prescribed by the Eclectics as expectorants in laryngeal and

bronchial diseases. They may be used in infusion, or better, in decoction. For its emmenagogue effect let the patient drink a teacupful of the warm tea of the root (3j to Oj) every two hours.

Perhaps there is no Composite of medical value which grows more abundantly than Wild Chamomile (*Maruta Cotula*) in this part of the State. This is the common Mayweed, known in some places as Dog Fennel. Its physiological action and its therapeutic uses are similar to Chamomile (*Anthemis Nobilis*), and it may be substituted for that remedy. The remedy is best administered in the form of an infusion. The bruised plant, or the infusion, have been recommended for ulcers. The application is said to cleanse the part and restore the normal color. Mayweed has been thought to be emmenagogue also. The flowers are more pleasant for internal use; but the entire plant is active.

Tanacetum Vulgare (Common Tansy), of this order, is very common in Southern Illinois, escaped from gardens. The leaves and tops are used. The remedy is also emmenagogue. It is tonic, deobstruent and anthelmintic. Dose, ʒss to 3j. The infusion may be used as a tea. The seeds are used as a vermifuge. The acid—tanacetic or tanisic—is prescribed for the same purpose. The remedy has been recommended in epistaxis, the odor being sufficient to check the hæmorrhage sometimes (Dr. C. P. Uhle.) Tansy has been used in intermittent fever, arthritic paroxysms, hysteria, and amenorrhea with asserted success. Tanacetic acid is equal to santonin in efficacy as an anthelmintic, and may be given in the same dose. "Tanacetic acid may be obtained by concentrating to the consistence of honey the residue after the distillation of the tops, heating with chalk and animal charcoal, and evaporating. If the residue be stirred with water, acidulated first with muriatic, and afterward with acetic acid, colored crystals of tenacetic acid are obtained, which are to be washed with distilled water."

The tansy tea is often given to girls who have "taken cold," where the menses are suppressed.

Of the Composite *Lappa Officinalis* (Burdock), there are three varieties in Southern Illinois (L. Major, L. Minor, L. *Tomentosa*.)

Burdock is a common domestic remedy. It is chiefly used as

a diaphoretic; it has value, however, as an aperient and as a diuretic. It has been recommended to the profession as valuable in cutaneous diseases; as herpes, lepra, psoriasis, prurigo, acne, and the skin diseases of children. I know the case of a boy who was suffering with itch (scabies), and determined to try what virtue there was in Burdock. He gathered quite a large quantity of the root of Common Burdock (*L. Major*), made a strong decoction of it, put it into a tub and bathed all over. In a few minutes he was covered all over the body with great wheals, which gave him intense itching, burning and pain. However, in a few hours these symptoms passed away, and with them the itch disappeared. The treatment was severe, but it produced a radical cure.

The remedy has been recommended also in nephritic affections, and in rheumatic, gouty, venereal, scorbutic, leprous, and scrofulous troubles. It has been used as a wash for chronic ulcers. The decoction is the preparation generally used.

Monotropa Uniflora (Indian Pipe, Corpse-Plant) is said to have been used in decoction as an alterative, or a tonic, by the common people of this section of the State. I have been told that formerly it was used to a considerable extent for this purpose in "biliousness." I have made a study of the plant, and am not aware that any other physician has ever made it the subject of investigation. Indian Pipe belongs to the Heath family (*Ericaceæ*), order *Ebenaceæ* (Ebony family.) The Persimmon, of opossum fame, belongs to this family. This tree, *Diospyros Virginiana* (Common Persimmon, Date-plum, American Date), grows in great numbers in our section. It has been used in diarrhœa, dysentery, uterine hæmorrhage, intermittent fever, sore throat, and by the people for making beer. The fruit was used formerly for making beer to a greater extent than now. The bark has been used in intermittents, and for ulcerated throat only, so far as I know. It may be given in the form of a decoction. For diarrhœa, dysentery and hæmorrhage it may be prescribed (that is, the fruit) as syrup, tincture or infusion. The green fruit should be used, as during the maturing process the fruit undergoes some change by which it loses its astringency.

Another genus of this large family is *Taraxacum*. *T. dens-*

leonis (Common Dandelion) is abundant in pastures and fields. This is a valuable remedy for such a region as ours, where the liver and spleen are generally involved in the various diseases with which we meet. It is especially in hepatic and splenic affections—engorgements—that it manifests a curative action. It may be used in both acute and chronic diseases of the stomach, liver and bowels.

The last plant of this order that I shall mention is Wild Lettuce. *Lactuca Canadensis* contains at least two varieties found in Southern Illinois (*L. integrifolia*, *L. sanguinea*.) It has been asserted by some physicians to have properties similar to garden lettuce (*L. sativa*), but this is denied by others. The actions which have been claimed for it are anodyne, diaphoretic and diuretic. I have not used it. The only plant of the *Lobeliaceæ* that I desire to mention is Indian Tobacco or *Lobelia* (*L. Inflata*.) This poisonous herb is used largely in domestic practice and by quacks, but it should not be confided entirely to their keeping. It should not be used as an emetic; but as an expectorant in asthma and bronchial diseases, cautiously administered, it is valuable. It is similar to Tobacco (*Nicotiana Tabacum*) in its action.

The Plantain family (*Plantaginaceæ*) is well represented in Southern Illinois. *Plantago Major* (Common Plantain) is a remedial agent of some value. It has been recommended for internal use in doses of one to four fluid ounces of the expressed juice. It is considered astringent, deobstruent, diuretic and refrigerant. The ancients used the remedy in visceral hæmorrhage and obstructions. I am only acquainted with its external use. It is freely used as a poultice in all kinds of ulcers and local inflammations, whether acute or chronic; in scrofulous tumors, and for dressing blisters. In modern times, as well as among the ancients, it has been used in consumption, dysentery, and intermittents, internally. The decoction may be used for either internal or external purposes. Externally, the leaves may be applied whole, sometimes bruised, or withered by dipping in hot water.

The Catalpa, of the *Bignonia* family (*Bignoniaceæ*), is represented by *C. bigonisides*. This tree, though not abundant, yet

grows in some places in considerable numbers. Many of them grow near Grayville, and are valued as fencing material. Their medicinal value is not so well understood. The *Catalpa* (Indian Bean) has been used by some European physicians in asthma. A decoction of the seeds is recommended. The fruit, a long bean, is generally considered to be poisonous. I have not used the remedy.

Verbascum Thapsus (Common Mullein) is the only plant of the Figwort family (*Scrophulariaceæ*) of medical interest that grows in Southern Illinois. Its properties are demulcent, emollient, and anodyne. It has been used in chest troubles. The tea is used in domestic practice. An infusion of the flowers has been used in mild catarrhs. A decoction of the leaves is said to be useful in diarrhœa. The dose of the infusion is one or two wineglassfuls. The mullein leaves are used in this vicinity as an anodyne poultice; this is the only use of the plant that I have made. The leaves should be dipped in hot water before using for external application.

Mentha Viridis (Spearmint), *M. canadensis* (Wild Mint), *Hedeoma Pulegioides* (American Pennyroyal), *Salvia Lyrata* (Lyre-leaved-sage), *Monarda Punctata* (Horse-mint), *Nepeta Cataria* (Catnip), of the Mint family (*Labiatae*), all grow in this region, and are used as domestic remedies, generally in the form of tea, for colds and colic. Sage tea is a favorite mouth and throat wash in ulcerated conditions of these parts. Many mothers use them freely for their daughters in dysmenorrhœa. Horehound (*Marrubium Vulgare*) is used in the same way, more especially in colds.

The Nightshade family (*Solanaceæ*) is represented in Southern Illinois by *Solanum Dulcamara* (Bittersweet), *S. Nigrum* (Common Nightshade), *Physalis Pubescens* (Ground Cherry), *Datura Stramonium* (Jamestown Weed, Thorn-apple), all of which have medical interest. Common Nightshade (*S. nigrum*) is a frequent domestic remedy, in the form of a poultice of the leaves and plant, to bruises, sprains, eruptions due to poisoning, etc.

Datura Stramonium is used as a poultice, domestically, over boils, carbuncles, painful tumors and local inflammations, bruises, stings of insects, etc. The latter remedy is very abundant, and

the physician frequently finds it convenient to make use of this plant. The leaves are generally used for poultices. The Ground Cherry may be used in the same way, but is not so efficacious. The fruit is edible.

I do not know of any other application or use of any of the Solanaceæ not described in the books.

Frasera Carolinensis (American Columbo), of the Gentian family (Gentianaceæ) is found in this part of the State. It is also called *F. walteri*. This is a simple bitter, and the infusion was so used formerly by physicians and people in domestic practice. The root is the part used. It may be employed in powder or decoction. I obtained my information concerning this plant from the late Dr. C. R. Smith. I have not seen it.

A valuable remedy of the Loganiaceæ (*Logania* family) which grows in this section is *Spigelia Marilandica* (Pink-root). It is used in domestic practice, and sometimes in general practice, in the form of a tea, against the round worm (*Ascaris Lumbricoides*.)

ARTICLE V.

MALARIAL KERATITIS. By F. C. HOTZ, M.D., Chicago, Ophthalmic Surgeon, Illinois Charitable Eye and Ear Infirmary.

Malarial diseases are so frequent, that it is singular how little we know of their effect upon the eyes. The most of text-books say nothing about it; only Macnamara * mentions malaria as a frequent and important cause of hyperæmia of the retina.

The fact is, we have but begun studying the very interesting subject of the influence which may be exercised upon the tissues of the eye by malarial poison or other febrific agents, after their introduction into the blood.

Directly bearing upon this question was the interesting communication which Prof. Horner, of Zurich, made to the German Ophthalmological Society in 1871.* He had observed the erup-

* Manual of Diseases of the Eye; 3rd Ed., 1876. p. 390.

* See Klein. Monatsbl. 1871.

tion of herpetic vesicles on the corneæ of thirty-one persons afflicted with pneumonia or other acute affections of the respiratory organs. Subsequent observers confirmed Horner's statement; but at the same time they found that other febrile diseases (intermittent fevers, acute affections of the intestines, etc.) may also produce herpes of the cornea. A very large collection of cases of this "herpes febrilis corneæ," as it is called, has recently been published by Dr. Godo, of Paris.* Referring to malaria as a cause of corneal herpes, he says that in at least one-third of his cases he could trace the affection of the cornea to the influence of malarial fevers.

But the descriptions of the cases which Dr. Godo reports as instances of malarial herpes, differ in no essential feature from those of his herpes cases due to other causes. It would be impossible to make a differential diagnosis from the local manifestations which are described, alike for all cases, as groups of minute vesicles in the peripheric cornea, which subsequently are transformed into superficial irregular ulcers, healing readily under the appropriate local and general treatment.

It was left to the careful and patient observer, Dr. Chas. J. Kipp, of Newark, to discover the peculiar and characteristic features by which keratitis due to malaria can be readily recognized, and easily distinguished from similar corneal troubles. The description the doctor furnished to the American Ophthalmological Society in July, 1880, is so true and clear that I may be allowed to reproduce his own words:

"If the eye is examined shortly after the first symptoms of irritation are noticed by the patient, one or two or more slightly raised opaque lines, of varying length, will be found on different parts of the surface of the cornea. At the same time, some circumcorneal injection will be present. On the following day these opaque lines will have increased in length, whilst at the same time the middle portion of the opacity has been transformed into a shallow ulcer. Under favorable circumstances, no further increase in size takes place, the remaining opaque epithelium is thrown off, and reparation begins, to be completed only after several months. But not unfrequently the ulcer continues for

* De l'hérpès fébrile de la cornée. *Recueil d'ophtalmologie*, Mars, Avril, Mai, 1880.

days, and even weeks, to grow slowly in length, and at the same time sends out club-shaped, slightly raised, grayish offshoots from its sides. In some of my cases the ulcer crept across the entire cornea, and in a few others, in which several ulcers appeared simultaneously on different parts of the cornea, the whole epithelial layer was eventually destroyed. As a rule, the ulcer shows no tendency to increase in depth, and its floor and edges are usually of a bluish-gray tint. The middle and inner layers of the cornea generally remain transparent throughout, but in neglected or maltreated cases, an extensive star-shaped opacity of a slightly yellowish-gray tint is sometimes developed in the inner layers of the central part of the cornea. An hypopyon is but rarely seen, even in the severest cases, and spontaneous perforation of the cornea did not occur in any of my cases. A decrease of the tension of the eye-ball, or a diminution of the sensibility of the cornea, could not be demonstrated in any of the cases. The development and growth of the ulcer was always accompanied by very severe pain in and around the eye, more especially along the course of the supra-orbital nerve, and by photophobia and lachrymation. The process of repair was commonly initiated by the extension of blood-vessels from the limbus conjunctivæ toward the ulcer, and was in all cases extremely slow, two to four months being generally required for its completion."

Should any one find it difficult to conceive from Dr. Kipp's description a distinct image of the peculiar form of these malarial ulcers of the cornea, let me assist his imagination by the aid of some familiar object in nature. Draw within the compass of the cornea the outlines of a small lanceolate leaf, with its stem at the margin and its free end in the center of the cornea; have the central vein of this leaf run in a slightly zigzag course, and let the lateral veins be short. Now erase the outlines of the leaf, and the skeleton of the veins is a correct representation of the specific character of a malarial ulcer of the cornea. This form of the ulcer is as pathognomonic for malaria as the mucous patches in the mouth are for syphilis.

You will now appreciate the great importance of Dr. Kipp's discovery. It has revealed to us the signs by which we can

recognize the constitutional cause of this form of corneal ulcers ; it has rendered our diagnosis independent from the statement of the patient. We can positively diagnosticate the malarial character of the ulcer, whether the patient admits or denies having had malarial fever. He who is not familiar with Dr. Kipp's specific symptoms, will probably regard the trouble as a simple keratitis or a superficial abrasion of the cornea, which he expects to relieve in a few days by local remedies ; but he will be sadly disappointed by finding that the ulcer has been daily increasing in length, and the pain, photophobia and other symptoms have not been subdued by the liberal use of atropia and the bandage.

Local treatment alone is insufficient. Starting from the margin of the cornea, the ulcer will extend longitudinally to and beyond the center of the cornea, and eventually also become broader by implicating the epithelium between the lateral offshoots. But give your patient quinine, and you will arrest the ulceration at any stage of its progress, and speedily relieve the patient of a very painful and troublesome affection. The amelioration supervenes upon the administration of quinine (two grains every two hours) so promptly that it must be attributed to the influence of this medicine. And when the surgeon has faithfully but vainly tried for a week or two to subdue the trouble by appropriate local treatment, the sudden change following directly upon the use of quinine is so suggestive and striking as to convince even the most skeptical Thomas.

A few cases may serve as illustrations to the foregoing remarks.

I. C. W., aged twenty-six, short-hand writer, consulted me on Jan. 24. Five days previously he had a chill followed by high fever at night. The next day his right eye became red and painful, and has been growing worse ever since.

I found the eye extremely sensitive even to moderate light ; and every movement of the upper lid caused a sharp pain as if a gritty substance was cutting into the cornea. The tears were constantly running over the lower lid and the eye-ball showed considerable engorgement of the conjunctiva. In the cornea, near its upper margin, I discovered a minute linear abrasion of the epithelium. I prescribed the use of an ointment of atropine

(gr. ij to 3j of vaseline), and dismissed the case. But on Jan. 29 the patient returned, stating that the treatment had given him no relief; that the pain had continued day and night, and that his sight had become very dim. The examination showed me that the patient was right, and had just grounds for his dissatisfaction. The inflammatory symptoms were as severe as before; and the abrasion of the epithelium had advanced downward across the center of the cornea. It formed a peculiar zig-zag line with short gray lines branching out alternately from each side at short intervals. Recognizing the significant character of this peculiar figure I at once gave the patient quinine. On the evening of the same day the patient already experienced great relief; the pain subsided, the profuse secretion of tears ceased, and he could open and move the eye without the slightest discomfort.

I saw the patient again on February 5, when the eye was free from all irritation and redness; the epithelium of the cornea was restored, but in its center there was a small star-shaped opacity which greatly impaired the sight. This opacity gradually grew smaller, but it was still perceptible when I saw the patient a few months since.

II. M. Br., aged thirty-five, conductor on railroad, came under my care on January 20. One week ago he had an attack of fever, which was followed by severe pain in the right side of the head and in the right eye. He could not open the eye, because every attempt caused a sharp cutting pain, and ordinary day-light was intolerable. The tears were running down the cheek in a continuous current. The ocular conjunctiva was engorged, and there was decided pericorneal injection. In the cornea, near its upper margin, a small abrasion of the epithelium was all the lesion I could discover. I did not notice anything unusual about this abrasion at that time, and prescribed atropine ointment and a bandage. After one week the patient returned. He had suffered great pain all the time, but especially at night it was exceedingly severe. He had lost his appetite, and his tongue was coated. The abrasion in the cornea had increased in length so as to extend almost across the entire cornea; it exhibited, then, very distinctly Kipp's characteristic symptoms; and, therefore, quinine was given at once. On the evening of the same day already

the pain had subsided, and the patient could sleep well. On the following day I observed a marked decrease of all the inflammatory symptoms. Three days later there was but a faint redness around the cornea; no lachrymation or photophobia. The epithelium was repaired along the line of ulceration, but its peculiar figure was still plainly outlined by an opacity. In the course of six weeks this opacity was cleared up.

Inasmuch as these opacities are slow in clearing, and particularly tenacious in the central portion of the cornea, an early recognition of the nature of this corneal trouble is of paramount importance, because the appropriate treatment can arrest the ulcerative process before it encroaches upon the center of the cornea. This point will be well illustrated by the following case:

Mrs. B., aged thirty-six years, had intermittent fever at different times. On the evening of Feb. 15, while in a warm room, she had a chill, followed by high fever. Next day, soon after breakfast, her left eye became very red and painful, and the tears kept running down the cheek. It was an intense, sharp pain, aggravated by the slightest movement of the eyeball or upper portion of the lid, so that the lady thought it was caused by a foreign body under the eyelid. Instead of a foreign body, however, I found in the upper nasal section of the left cornea, very near its margin, a minute, slightly raised, gray spot, like a phlyctenule, from which a fine gray linear opacity proceeded downward. This gray line was only one millim. long, but the most valuable symptom in the case; for at its lower end it branched out, thereby indicating the malarial keratitis in its incipient stage. I prescribed quinine (two grs. every two hours), and put a drop of atropine in the eye. After three doses of quinine were taken, the pain began to subside. The next day lachrymation, photophobia, pain and redness had disappeared, and two days later no trace of the corneal trouble could be discovered.

A PHYSICIAN of Erie, Pennsylvania, is training carrier pigeons for use in his practice. He leaves pigeons at places from which he wishes reports to be sent him.—*Boston Med. and Surg. Journal.*

Clinical Reports.

ARTICLE V.

FISTULA UPON THE FACE. By EUGENE S. TALBOT, M.D., DD.S.
Clinical Lecturer on Dental and Oral Surgery at Central Free
Dispensary, Rush Medical College. Reported by J. LONG.

This patient, Miss Sarah K——, has kindly consented to appear before you, in order to furnish an opportunity of studying an important lesion. I trust her case will so impress itself upon your minds as to assist you in treating like cases intelligently.

This young woman is an American, sixteen years of age, and in perfect health. Her parents are both living and healthy. She has been suffering from this trouble six years. It first manifested itself while the patient was living in New Orleans, by pain and swelling on the side of the face. A physician was summoned, who made an incision at the border of the jaw, anterior to the facial artery, where he had discovered pus. The pus discharged itself in large quantities, but the wound did not heal; pus constantly trickled out of the opening, and a permanent fistula was established. Three years afterward the family moved to Chicago, and the girl was treated by a physician who probed and burnt it at intervals for two years. At the end of that time, there being no improvement, she went to one of our hospitals, and while under the influence of ether an operation was performed, and the wound sewed up. When this failed to heal, she applied to a physician, who assured her the previous treatment was hasty; that she was scrofulous, and needed constitutional treatment. Last June, while under his care, she came into my office with a patient of mine. As I was about to dismiss my patient, my attention was called to her friend's disfigured face.

I requested her to open her mouth. Upon doing so, I found only the roots of the first and second molars posterior to the bicuspid, upon the right side of the inferior maxilla. I informed her that a cure could be effected by a removal of the roots. As she had not prepared herself for an operation, she postponed it for a time.

Two weeks ago she appeared again, and desired me to take the roots out. Upon removing the anterior root of the second molar, I found an abscess, the cause of all her suffering. You observe that the fistula has now entirely healed. A swollen face is indicative of bad teeth, so we can presume this trouble proceeds from the teeth, before further considering the condition of the health.

Observe this ugly contracted scar upon the border of the jaws, and question yourselves if it is ever justifiable to puncture an abscess upon the face. After ascertaining the offending tooth, if badly decayed, or a root, remove it, that the abscess may discharge itself through the opening in the jaw. If, for any reason, you should wish to lance the abscess first, open the mouth and draw the cheek out with the finger, then pass your bistoury between the mucous membrane and the bone, the blade resting against the jaw, thus avoiding the facial artery. The pus will pass into the mouth, and prevent the scarring of the face. Should a fistula be already established upon the face, examine the mouth, and if you succeed in finding the seat of the lesion, remove the tooth or root, and nature will heal the wound.

BEACONSFIELD'S DOCTORS.—Dr. Quinn has been called upon to defend himself before the College of Physicians for consulting with Dr. Kidd, who is an eclectic practitioner. Dr. Quinn explained that before seeing Lord Beaconsfield, he received a letter from Dr. Kidd, saying that he was not treating the case homœopathically, and that every direction and prescription of Dr. Quinn's would be faithfully carried out. From a letter of Dr. Kidd to the medical journals it appears that Sir William Jenner absolutely refused a consultation prior to the summoning of Dr. Quinn.—*Medical News*.

Society Reports.

ARTICLE VI.

CHICAGO MEDICAL SOCIETY. Stated Meeting, Oct. 17, 1881.

Dr. E. Ingals, President, in the Chair.

Dr. E. L. Holmes presented an abstract of the following remarkable cases, reported by him in Knapp's Archives of Ophthalmology :

DR. HOLMES' PAPER.

BLINDNESS—COLOR BLINDNESS DURING PREGNANCY.—Mrs. —, thirty-one years of age, who had always enjoyed excellent health, became quite suddenly ill in the sixth month of her first pregnancy. In the course of two weeks there appeared successively symptoms of albuminurea, pericardial effusion, orthopnœa, slight but quite general œdema, and total blindness. At this stage of the disease I was requested to make an ophthalmoscopic examination.

There was an extensive neuro-retinitis of each eye. The vessels of the retina and discs, as well as the contour of the discs, were almost entirely hidden from view by exudation. The discs themselves could only be recognized as quite white flocculent clouds. At the temporal side of the right eye was a detachment of a portion of the retina. There were dilatation of the pupils and a partial paralysis of the internal rectus of the eye.

As I had often observed total atrophy of the optic nerves and total blindness in cases with a history similar to this, I could only express a doubtful prognosis as regarded the restoration of sight.

The physicians in attendance urged the induction of premature labor. I recommended the use of tinct. ferri mur., pot. iod.,

pot. bromide and hyd. bichlor, in suitable doses, but did not see the patient again during her illness.

Six years after this I had an opportunity of examining this patient. The left eye was almost without abnormal appearance. Vision was perfect. In the right eye, at the seat of the detachment of the retina, was a small area of pigment deposit. Central vision was nearly extinct, with great obscurity of the surrounding field.

The patient informed me that for a period the remedies which I had advised had been taken, and subsequently strychnia. During three weeks she had been totally blind, and color blind three months. Gradually she was able to distinguish white and black, then yellow, red, brown, and finally green. Not till the end of five years was there perfect faculty of perceiving tints of green.

EYE-LASH IN THE ANTERIOR CHAMBER.—A German farmer thirty-five years of age, who had lost his right eye in childhood, received a punctured wound of the left cornea from a short piece of straw thrown from a threshing-machine. Blindness of the eye was the immediate result, occasioned probably by hæmorrhage in the anterior chamber. Little pain and inflammation followed the accident.

On examination, nine months after the injury, I observed a small opacity of the cornea at its upper and outer section. The lens was cataractous. Behind the opacity of the cornea was a rent in the iris. At the temporal portion of the anterior chamber was an eye-lash, which had evidently been carried through the cornea by the piece of straw. This was easily removed by means of a small blunt hook, through a minute incision in the cornea. A subsequent removal of the cataract restored excellent vision.

Dr. Holmes also reported the following case of anchylo-blephor-
an :

Last summer he was called to see a child four years old, in whom both eyelids were so covered with sores that it seemed impossible as well as inexpedient to separate them. The child had been vaccinated in the latter part of the winter; from that time herpetic sores had broken out all over the skin; but to what

extent the vaccination was accountable for the disease, Dr. H. could not determine. The patient presented all the indications of scrofula, and was put on appropriate treatment. He saw the case a few days ago, and noticed the following conditions: The skin had been restored, but the eyelids had not been separated. These he parted by inserting the points of scissors into a small incision made with a small bistoury between what had been the edges of the lids. The cornea of one eye was opaque. in the other quite transparent. There was some photophobia, Dr. Holmes remarked that sewing the eyelids together, and in due time separating them again, are recommended in xerophthalmia.

Dr. Hotz had met with cases of neuro-retinitis and blindness complicating pregnancy. Sight was restored in many cases, and as to the prognosis, he believed that it was much more favorable whenever blindness was dependent on the albuminous retinitis of pregnancy than on that of Bright's disease; and in the latter case it was always a precursor of death.

Dr. R. Tilley reported a case of strabismus depending on inflammation of the middle ear, which ceased after the discharge from the ear had stopped.

Dr. Holmes gave his assent to Dr. Hotz' remarks, but he added that rather a large proportion of cases of blindness from pregnancy had come under his observation which did not recover.

Dr. Hotz reported some cases of wounds of the cornea by straws from threshing-machines. It was a most frequent cause of injury among farmers. The pieces of straw should always be extracted, even when the cornea had to be cut into for that purpose, and the use of chloroform was advisable when required.

Dr. Ingals asked of the ophthalmologists present, what was the cause of asthenopia after labor? He had always supposed that it depended on straining of the ophthalmic nerves in the straining of labor, and on the congestion then liable to be present.

Dr. Holmes did not think that it was owing to congestion as much as to temporary weakness of the muscles of accommodation following exhaustion of the system at large. But this explanation was not satisfactory to the enquirer.

Dr. Tilley related an interesting case in a girl sixteen years of

age, who had not opened her eyes for two months when he first saw her. She had a tumor of the mammary gland, which was removed, and found to be a cystic sarcoma. Just as she recovered from under the influence of the anæsthetic, she opened her eyes. After four weeks, the same trouble returned, and the patient was again anæsthetized, with the same result. After two months there was marked internal strabismus, which was overcome by straining the eye in the opposite direction repeatedly.

Dr. Emma F. Gaston, six weeks ago, had seen a lady in apparent good health, who was suddenly attacked by irritation of the eyes while paring peaches. The inflammation had subsided, under proper treatment, in three days. Was there something irritating or poisonous about the peaches ?

ARTICLE VII.

MICHIGAN STATE BOARD OF HEALTH.

The regular quarterly meeting of this Board was held Oct. 11, 1881. An interesting feature was a report by the Secretary relative to work of other State Boards of Health. The Secretary of the Michigan Board desires to continue to receive information from other Boards, by which these reports may be made quarterly.

A report relative to work of local boards of health showed increased activity on the part of local health authorities in the way of isolating those affected with communicable diseases, and enforcing the law requiring from householders and physicians notices of such diseases. In one city a physician had been fined \$100 for not reporting cases of diphtheria.

The revised document on the restriction and prevention of scarlet fever was adopted, and ordered published in English, Dutch and German. The consideration of this document involved a discussion of the question of recommending health officers to verify diagnoses of reported cases of diseases dangerous to the public health.

A circular giving general rules for the prevention of diphtheria, scarlet fever and small-pox was adopted. Forms were

adopted for annual reports by health officers and clerks of local boards of health, and by regular correspondents of the Board.

Dr. Avery, of Greenville, was requested to visit the overflowed district along the Maple river, in Gratiot county, and report to the Board.

Dr. Lyster, of Detroit, read a paper on syphilis in its relations to the public health. It dealt with the facts of the frequent communication of the contagium of syphilis, by direct and by indirect means, to innocent persons; also with the serious effects on individuals, and on the offspring of marriages, where one of the parents is thus blighted. He believed much might be done toward preventing this loathsome disease by wise legislation which shall restrict syphilis, and especially by collecting and disseminating among young men and other people facts relating to the nature and dangers of this disease.

Dr. Kellogg read a paper on the Relations of Preventable Sickness to Taxation, showing, by the reports of the boards of correction and charities, the abstracts of reports of county superintendents of the poor, the abstracts of statistical information relating to the insane, and the deaf, dumb and blind, and the vital statistics report, that more than 3,000 persons in Michigan are annually dependent on the State for support to a greater or less extent, in consequence of diseases preventable by the adoption of proper sanitary measures. The cost to the people of the State for the support of these persons is over \$40,000 annually, a portion of which is paid by every tax-payer. This is but a small part of the actual loss to the State. The number of deaths from preventable sickness in 1880 (*estimated* from returns by supervisors and assessors), was 4,585. Placing the value to the State of each human being at the low estimate of \$1,000, the aggregate loss by deaths from preventable sickness is over \$4,500,000. But to this must be added a further loss from sickness which did not terminate fatally. The statistics of the benefit societies of England show that for every person who dies three persons (on the average) are sick, throughout the year. This indicates a total annual loss of time from preventable illness on the part of more than 9,000 persons, to which should be added the expense of living, etc., certainly more than \$1,000,000. This gives about

\$5,666,000 as the total loss to this State from diseases generally conceded to be preventable. These figures are regarded as much too small, because of the few diseases included in this estimate as preventable (though it is generally conceded by sanitarians that at least nine-tenths of all ailments may readily be prevented), and because only sickness and deaths directly traceable to preventable causes have been included, while a large amount of sickness and many deaths are indirectly due to these causes. It is probable that preventable sickness might justly be charged with an expense to the State of not less than ten million dollars. Estimating the loss in the other States in the same ratio to the population, the aggregate loss to the whole United States is not less than three hundred million dollars annually, an amount which would pay the national debt in six years.

Mr. Parker, of Flint, presented a report of the public health section of the American Social Science Association at Saratoga.

The committee on sanitary survey of the State was requested to prepare schedules for the sanitary survey of cities, villages and townships.

Mr. Parker reported a proposed bill authorizing all boards of education to exclude from school persons infected with diphtheria, scarlet fever or small-pox, or living at houses where any person is infected with one of these diseases.

The Secretary was directed to prepare and issue a weekly bulletin of sickness in Michigan for such papers and medical journals as will publish it.

Dr. Baker was authorized to procure the services of an architect in the preparation of a circular on hospitals for communicable diseases.

Dr. Kellogg reported on the subject of criminal abortion. He and Dr. Hazlewood were requested to prepare a circular designed to collect facts on this subject.

ARTICLE VIII.

AMERICAN DERMATOLOGICAL ASSOCIATION: FIFTH ANNUAL MEETING. Held at Newport, R. I., August 30 and 31, and Sept. 1, 1881. Official Report of the Proceedings by the Secretary, Dr. ARTHUR VAN HARLINGEN, of PHILADELPHIA.

[CONCLUDED FROM NOVEMBER NUMBER.]

SECOND DAY.—MORNING SESSION.

DR. ISAAC E. ATKINSON, of Baltimore, read a paper entitled

A Case of Tubercular Leprosy,

which was supplemented by a paper by Dr. I. Bermann, of Baltimore, upon the presence of the "bacillus lepræ" in the leprous infiltrations removed from the patient. The patient, a woman about 45 years old, was born of healthy parents, in Baltimore, and had never been beyond the limits of Maryland. She was married at sixteen. Her illness dated back about five years, and had followed a course typical of tubercular leprosy. The most interesting feature of the case was the probability of its having been acquired through contagion. This is the only case upon record of leprosy acquired by one who, born in Maryland, has never been beyond its limits. It can hardly, therefore, be an accidental coincidence that this woman, living for some time next door to a leper, whose case has been recorded by Dr. Rohé in the *Maryland Medical Journal* (1878), should get the disease without deriving it from him. It should be remembered that this man, who probably acquired his leprosy in Cuba, a young man, a native of Barbadoes, and a woman who probably acquired her disease in Louisiana (all reported by Rohé), comprise all the lepers that have been reported in Maryland for a number of years. It is evident that leprosy is one of the rarest of maladies in that State. The objection to evidence adduced in support of the contagiousness of leprosy has heretofore been that it has been collected in countries or localities where the disease prevails as an epidemic, and might arise from other causes. Here, however, is a case where the contagious influence was possible in a locality where leprosy not

only does not prevail, but has never, previously, been known to affect one whose life has been spent within its limits. Such conditions have never before been reported. The researches of Dr. Bermann into the histological pathology of the tubercles removed from the ear of this patient, enabled him to confirm the discoveries of Hauser, Neisser, Eklund, and others, of a bacterium as a constant attendant and probable cause of leprosy. Aniline colors being best for demonstrating bacteria, Bermann followed the prescribed methods and detected in the nuclei of the cells little bodies corresponding exactly with the bacillus lepræ. The process giving the most perfect results was that of staining the section in a one per cent. solution of aniline blue for a few seconds, washing in absolute alcohol several times, transferring to oil of cloves, again to the alcohol, then back to the oil of cloves, and finally mounting in balsam. The result of this method is, that the pigment is nearly removed from the protoplasm of the cell and leaves the bacillus deeply stained. Several prepared sections were exhibited.

On motion, it was agreed to postpone the discussion on Dr. Atkinson's paper until the other papers on the same subject should have been read.

DR. J. NEYINS HYDE, of Chicago, read a paper entitled

Study of a Case of Acute Tubercular Leprosy.

This paper was illustrated by a portrait in oil, representing the face of the patient.

DR. HYDE then read a paper by DR. H. D. SCHMIDT, of New Orleans, entitled

The Pathology of Leprosy.*

DR. LOUIS A. DUHRING, of Philadelphia, was announced to read a paper by MR. WM. FLETCHER, B.A., of Canada, entitled

Cases of Leprosy in Cape Breton, Nova Scotia.

Dr. Duhring apologized for the non-reception of Mr. Fletcher's communication, which had miscarried in the mail.

On motion of DR. WHITE, of Boston, it was agreed that Mr. Fletcher's paper be referred to Dr. Graham, of Toronto, the member of the Committee on Statistics in whose district the

* This paper will appear in the *Archives of Medicine*, October No.

cases had occurred, in order that it might be incorporated with the report of the committee.

SECOND DAY.—EVENING SESSION.

Discussion on Leprosy.

DR. WHITE, of Boston, said he was becoming more and more convinced of the contagiousness of leprosy through inoculation. The rise and spread of the disease in the Sandwich Islands seemed convincing as to contagiousness. He asked Dr. Atkinson if he could explain how contagion had occurred in his case.

DR. ATKINSON, of Baltimore, in reply to Dr. White, said that there were open sores upon the man B., which of course might be the means of contagion. Whether sexual intercourse had taken place between the parties he could not say, but every outward circumstance was against such a supposition. The woman was a person of the highest respectability.

DR. WHITE went on to say, regarding Dr. Hyde's paper, that he could not accept the case there reported as one of leprosy. The rapidity of development of the skin lesions was unprecedented. The picture accompanying the paper did not seem to him to present the appearance of leprosy. He would be more ready to accept the diagnosis after a careful microscopical examination of the lesions should have been made.

DR. GRAHAM, of Toronto, said that Dr. Keys, who had examined the lepers at Cape Breton, considered leprosy contagious only by inoculation. Drs. Bayard and Wilson were of the opinion that the disease is contagious.

DR. HEITZMANN, of New York, said, in reference to the microscopic preparations of leprosy made by Dr. Bermann and exhibited by Dr. Atkinson, that these specimens showed appearances which some observers considered parasites. The brief space of time available for the examination, and the method of mounting employed, prevented him from expressing a positive opinion, but he considered the supposed rods to be a part of the protoplasmic structure of the so-called cell. He thought staining agents delusive. As to Dr. Schmidt's elaborate paper, although this was valuable, it was based upon old-fashioned views of pathology. As we know that there are trophic centers every-

where in the central nervous system, and as many skin diseases appear in the light of recent investigations to be due to affections of the central nervous system, it was to this system, in his opinion, we must look for the origin of leprosy.

DR. DUHRING, of Philadelphia, said he had been much interested in hearing Dr. Atkinson's paper. In listening to the history of the case as given, he could see no point at which contagion could have taken place. The opinion of observers seems more and more inclining to the view of the contagiousness of leprosy. He agreed with Dr. White in thinking that contagion could only take place by inoculation. With regard to the case which had been reported by the President, Dr. Hyde, Dr. Duhring inclined to the belief that contagion could only have taken place by inoculation. With regard to Dr. Hyde's case, so far as he could judge from what he had heard of the history, he thought the diagnosis doubtful. He had never heard of a case of leprosy running such a rapid course. From what he had heard of the case, and especially from the accompanying picture, he was reminded of a case described by himself in a paper read before the Association a few years ago, under the name of "Inflammatory Fungoid Neoplasm." The color and appearance of the lesions in Dr. Hyde's case resembled closely those observed in Gubler's, Piffard's, and his own case.

THE PRESIDENT, DR. HYDE, of Chicago, said that he himself was not perfectly certain of the diagnosis. He should not come to a positive conclusion until a careful histological examination had been made of the lesions. He had at first regarded the case as one of that affection called by Dr. Heitzmann "myeloma," and of which Dr. Duhring's case was an instance. As to the acuteness of the disease, it must be remembered that it had in fact lasted a long time. Dr. Hyde then recapitulated the chief points in his description of the case, going to show that the lesions were characteristic to a considerable degree of leprosy.

DR. ATKINSON, said that the fortuitous concurrence of two cases, such as had been described in his paper, would seem almost impossible. The only case of leprosy which had ever originated in the State of Maryland was the one which he had described, and this had occurred in the immediate neighborhood of a former case. He thought it impossible to escape the conclusion that there was some connection between the

two cases, even though the history had failed to bring such connection to light. In his opinion, the hereditability of leprosy was doubtful. Regarding the question of the presence of "*bacillus lepræ*" in the microscopic specimens, he would only say that Dr. Bermann claims that he has found similar appearances to those described by Hansen and Ecklund, to which they gave that name.

DR. WIGGLESWORTH, of Boston, said that he had in his student days seen much leprosy, having traveled in Norway, Spain and Turkey for the purpose of studying the disease. He had, however, never seen a case of leprosy like the case described by Dr. Hyde. If Dr. Hyde could see Baretta's model of a case of "*mycosis fungoide*," he would see how closely it resembled his (Dr. Hyde's) case. Dr. Wigglesworth thought that many of these diseases might be grouped as lymphosarcoma, and went on to give the leading symptoms of Dr. Hyde's case and to point out the resemblance between it and lymphosarcoma.

DR. HYDE said he had been lead by reading Kaposi's paper on a case of acute leprosy to incline to this diagnosis in the case reported.

DR. DUHRING said he regarded Baretta's model, to which Dr. Wigglesworth had alluded, as representing the same disease as his inflammatory fungoid neoplasm. He referred to a case of leprosy which he had seen where the course was very obscure. It had occurred in a Cuban gentleman who had traveled much, and in whom it had shown itself at the age of thirty, the patient dying five years later.

DR. WIGGLESWORTH recalled the case of an American sailor who visited various ports of South America, and returning showed symptoms of leprosy seven years later. He thought the disease might have been contracted in this case by inoculation, by cohabitation, etc.

DR. EDWARD WIGGLESWORTH, of Boston, read a paper on

Buccal Ulcerations of Constitutional Origin.

These are usually syphilitic in nature, though they are not always so. The fact that syphilitic ulcerations sometimes occur in persons above all suspicion of sexual immorality makes it doubly important for the general practitioner to recognize them by their appearance without asking too many questions. The only maladies likely to occasion doubt in regard to diagnosis

are lupus and tuberculosis, and occasionally epithelioma. In cases of lupus the local treatment is identical with that adapted to ulcerations due to syphilis. When tuberculosis exists with ulcerations of the fauces or palate, or both, the pulmonary symptoms are almost always far advanced, and even when they cannot be detected the patient is sure to die of tuberculosis, more or less general, and treatment is practically useless. The possibility, however, that the case may be one or either of these diseases should always make the practitioner careful in expressing an opinion and considerate in his language, for the peace of a whole family is often at stake.

The writer then went on to give notes of a series of illustrative cases, and followed these by some general remarks on the treatment, particularly of syphilitic ulcerations. Stimulants are better than caustics, and, indeed, much harm is done by the indiscriminate use of the latter. The parts must be kept well cleansed, and this alone, when the ulceration is in the pharynx, gives great relief. The writer recommended the following solution for atomization :

℞ Tinct. iodini, pts. v ;
Glycerinæ, pts. x ;
Aquæ, pts. xxx.—M.

The most convenient instrument to use is that known as Fulgraffé's, which is made of hard rubber, with nozzles of various shapes—one to send a stream of spray up behind the soft palate, one for a horizontal stream, and one for use in the larynx. The bottle should be kept nearly full, so that the stream may not be kept up longer than is intended, owing to compressed air remaining in the bottle. Three or four applications of twenty seconds each will be sufficient to thoroughly cleanse and stimulate any ordinary ulceration.

After the use of the spray, iodoform should be applied to the ulcer with a camel's hair pencil, or blown on to it by an insufflator. A little powdered gum arabic mixed with the iodoform causes it to adhere better. Iodoform can also be conveniently deposited on the nasal mucous membrane by letting the patient inhale a saturated solution of the drug in ether.

Some cases require the use of the nasal douche with disinfectants. A convenient formula is one teaspoonful of chlorate of potassium with one-third as much carbonate of sodium and

fifteen drops of a five-per-cent. solution of permanganate of potassium, all in a quart of water at 25 degrees C., used with a fountain syringe. It is well to remember that a spray or a powder carried by a current of air may be used by blowing it into one nostril while the patient breathes through the open mouth. The spray then passes through the vault of the pharynx and escapes by the other nostril. If desirable, by instructing the patient to breathe through the nose, a powder may be blown into the larynx through the nostril.

Constitutional treatment is of course necessary. The lesions occurring late in the disease—from the second to the twentieth year, and usually in cachectic subjects—iodine must play a more important part than mercury. Cod-liver oil, ferric iodide, potassic iodide, iodine spray, and iodoform—these are the great remedies. Of course all hygienic measures must be united with these.

DR. F. B. GREENOUGH, of Boston, thought that although local treatment was beneficial in the class of cases described, yet keeping the parts clean, and constitutional (mixed) treatment was as good or better.

DR. WIGGLESWORTH said he thought a cure could be effected more rapidly by employing local treatment in addition to cleanliness and constitutional measures.

DR. GREENOUGH said he had found iodoform a very useful application. He had used it successfully in one case of epithelioma.

DR. HEITZMANN, of New York, asked Dr. Wigglesworth if he had not found much difficulty in distinguishing between syphilis and tuberculosis of the throat. He adduced several personal instances where the diagnosis was very difficult.

DR. WIGGLESWORTH said he often found the diagnosis between syphilitic and tubercular ulceration of the throat very difficult. In answer to a question by the President he said the form of iodide of iron he preferred was the syrupus ferri iodidi of the pharmacopœia.

DR. ATKINSON, of Baltimore, said he had been lead by a remark of Kaposi to attribute much weight to certain symptoms in the diagnosis of these affections; *e. g.*, that lupus is not so apt as syphilis to attack the bones.

DR. WIGGLESWORTH said that lupus does attack the bones, although, to be sure, not often.

DR. ATKINSON, continuing, said it was often difficult to check the ulcer of the uvula before perforation should have taken place. He considered constitutional treatment the most important in these cases. He had found most trouble in the effort to cure old recurring cases.

The PRESIDENT, DR. HYDE, of Chicago, in closing the discussion, added his testimony to that already given on the great importance of cleanliness. It was a *sine quâ non* in every case, whatever form of treatment might be adopted.

THIRD DAY.—MORNING SESSION.

The report of the Committee on Statistics was read as follows :

REPORT OF THE COMMITTEE ON STATISTICS FOR THE YEAR ENDING JUNE 30, 1881.

TABLE I.

District 1.—Boston Cases . .	{ Dispensary . . 2,558	{ Private . . 706	Total . . 3,264
District 2.—New York Cases .	{ Dispensary . . 3,295	{ Private . . 542	Total . . 3,837
District 4.—Philadelphia Cases	{ Dispensary . . 1,010	{ Private . . 98	Total . . 1,108
District 4.—Baltimore Cases .	{ Dispensary . . 503	{ Private . . 0	Total . . 503
District 5.—St. Louis Cases .	{ Dispensary . . 0	{ Private . . 264	Total . . 264
District 6.—Chicago Cases . .	{ Dispensary . . 1,069	{ Private . . 1,031	Total . . 2,100
Total Dispensary Cases . .			8,435
Total Private Cases . . .			2,641
			11,076

TABLE II.

Showing the Returns from the respective Districts.

	Private Cases.	Dispensary Cases.	Total Cases.	Boston.	New York.	Philadelphia.	Baltimore.	St. Louis.	Chicago.
Class I. Disorders of the Glands.									
1. <i>Of the Sweat Glands.</i>									
Hyperidrosis	4	14	18		11	3	1	1	2
Miliaria crystallina.....	9	11	20			6		3	11
Anidrosis	1		1						1
Bromidrosis.....	2	3	5						5
Chromidrosis									
2. <i>Of the Sebaceous Glands.</i>									
Seborrhœa: a. oleosa; b. sicca.....	82	163	247	83	56	30	17	6	55
Comedo.....	34	90	124	24	59	9		4	28
Cyst: a. Miliun; b. Wen.....	23	18	41	7	12	8		3	11
Molluscum sebaceum.....	10	10	20	5	5			1	9
Diminished secretion.....									
Class II. Inflammations.									
Exanthemata.....	107	122	229	16	28	3	6		176
Erythema simplex.....	29	58	87	10	31	8	5	2	31
Erythema multiforme: a. papulosum; b. bullosum; c. nodosum	28	96	124	40	40	26	5		13
Urticaria	42	302	344	75	174	54	8		33
* Dermatitis: a. traumatica; b. venenata; c. calorica.....	57	183	240	42	109	36	15	4	34
Erysipelas	32	103	135	33	56	6	3		37
Furuncle.....	48	183	231	50	106	25	12	6	32
Anthrax	8	1	9		3				6
Phlegmona diffusa.....	6	12	18	6	3		3		6
Pustula maligna.....									
Herpes: a. facialis; b. progenitalis.....	69	112	181	40	58	11	4	2	66
Herpes zoster.....	21	127	148	48	45	17	8	2	28
Psoriasis.....	92	250	342	83	128	47	11	4	69
Pityriasis rubra.....		1	1						1
Lichen: a. planus; b. ruber.....	5	21	26	1	12	2	3		8
Eczema: a. erythematous; b. papulosum; c. vesiculosum; d. madidans; e. pustulosum; f. rubrum; g. squamosum.....	623	2880	3503	1231	1226	370	201	77	398
Prurigo.....		1	1		1				
Acne	226	430	656	270	170	69	22	26	99
Impetigo	19	146	165	67	37	25			36
Impetigo contagiosa.....	8	52	60	12	1	23	6	1	17
Impetigo herpetiformis.....									
Ecthyma.....	5	65	70	33	24	5	1		7
Pemphigus.....	5	16	21	3	6	3	1		3
Class III. Hæmorrhages.									
Purpura: a. simplex; b. hæmorrhagica.....	11	43	54	8	24	7	2	1	12
Class IV. Hypertrophies.									
1. <i>Of Pigment.</i>									
Lentigo	3	4	7	4				3	
Chloasma: a. locale; b. universale.....	32	28	60	18	22		3	1	13
2. <i>Of Epidermal and Papillary Layers.</i>									
Keratosis: a. pilaris; b. senilis.....	15	8	23	10	2	2		3	6
Callositas.....									
Clavus.....		6	6	1	2	1	2		
Cornu cutaneum.....									
Verruca.....	41	67	108	37	26	8	5	4	28
Verruca necrogenica.....									
Xerosis	3	6	9	8				1	
Ichthyosis.....	7	14	21	2	6	3	2		3
Of Nail.....	6	3	9	1	2			3	1
Hirsuties.....	30	2	32	12	3			15	2
3. <i>Of Connective Tissue.</i>									
Scleroderma.....	2	3	5	2	2				1
Sclerema neonatorum.....									

* Indicating affections of this class not properly included under other titles.

Table showing the Returns from the respective Districts.

	Private Cases.	Dispensary Cases.	Total.	Boston.	New York.	Philadelphia.	Baltimore.	St. Louis.	Chicago.
Morphœa	5	5	5		3			1	1
Elephantiasis Arabum.....	5	5	4	1					
Rosacea: a. erythematosæ; b. hypertrophica.	82	77	159	22	74	19	2	14	28
Frambœsia									
Class V. Atrophies.									
1. <i>Of Pigment.</i>									
Leucoderma.....	4	6	10		6			1	3
Albinismus.....		2	2				2		
Vitiligo.....	7	13	20	6	1	6	3		4
Canities.....	2	1	3	2	1				
2. <i>Of Hair.</i>									
Alopecia.....	68	22	90	62	14	4	2	3	5
Alopecia areata.....	34	31	65	29	19	4	1	1	11
Alopecia furfuracea.....	47	15	62	44	4	3	4		7
Atrophia pilorum propria.....									
3. <i>Of Nail.</i>		3	3	1	1	1			
4. <i>Of Cutis.</i>									
Atrophia senilis.....									
Atrophia maculosa et striata.....	2		2					1	1
Class VI. New Growths.									
1. <i>Of Connective Tissue.</i>									
Keloid.....	4	11	15	3	4	2	4	1	1
Cicatrix.....	2	8	10	2	4		1	1	2
Fibroma.....		1	1						1
Neuroma.....									
Xanthoma.....	3	2	5	1	4				
2. <i>Of Vessels.</i>									
Angioma.....	21	26	47	16	7	3	4	4	13
Angioma pigmentosum et atrophicum.....	1		1		1				
Angioma cavernosum.....		1	1	1					
Lymphangioma.....	1	1	2		1	1			
3. <i>Of Granulation Tissue.</i>									
Rhino-scleroma.....									
Lupus erythematosus.....	12	17	29	7	10	2		1	9
Lupus vulgaris.....	15	36	51	8	13	8	2	6	14
Scrofuloderma.....	7	35	42	11	7	4	4		16
Syphiloderma: a. erythematosum; b. papulosum; c. pustulosum; d. tuberculosum; e. gummatosum.....	283	982	1265	273	454	85	23	16	414
Lepra: a. tuberosa; b. maculosa; c. anæsthetica.....	5	2	7	1	1		1		4
Carcinoma.....	36	51	87	21	27	10	6	5	18
Sarcoma.....		1	1		1				
Class VII. Ulcers.	48	385	433	97	202	19	24	5	86
Class VIII. Neuroses.									
Hyperæsthesia: a. Pruritus; b. Dermatalgia.	72	202	274	75	82	37	25	16	39
Anæsthesia.....	2	2	4				1	1	2
Class IX. Parasitic Affections.									
1. <i>Vegetable.</i>									
Tinea favosa.....	2	23	25	4	13	2	2		4
Tinea trichophytina: a. circinata; b. tonsurans; c. sycosis.....	91	258	349	84	115	22	20	11	97
Tinea versicolor.....	26	98	124	23	44	15	8	3	31
2. <i>Animal.</i>									
Scabies.....	7	127	134	22	64	11	6		31
Pediculosis capillitii.....	3	213	216	110	51	24	7		24
Pediculosis corporis.....	3	218	221	51	118	10			42
Pediculosis pubis.....	11	34	46	11	12	4			19

Special Report on Leprosy.

New Brunswick.—Dr. Graham, of Toronto, presented a brief history of the progress of leprosy in New Brunswick, drawn from the annual reports to the legislature of that Province for the last thirty years, with a valuable abstract of the report of a medical commission made in the year 1848 by Drs. Bayard and Wilson.

California.—Dr. Hyde presented the following account of the disease upon the Pacific Coast :

TWENTY-SIXTH ST. HOSPITAL,
SAN FRANCISCO, CAL.. JULY 10, 1881.

JAMES NEVINS HYDE, M.D.,

President Am. Derm. Association.

MY DEAR DOCTOR :—Your favor of last month found me busily engaged on the annual report, a fact for which I would have you credit me, as against my neglect in allowing your communication to remain so long unanswered. In regard to the subject of your inquiry, leprosy, I shall be but too happy to furnish you with all the data attainable from our records, at the same time would remind you of the fact that the subjects of the disease, with one exception, have been Mongolians, and unable to give other than the most meagre account of themselves, and we are often compelled to rely upon conjecture in fixing the age of the patient.

I find by reference to the records that the first leper, *Hoy Tong*, was admitted July the 5th, 1871, and that he died on 29th of Sept., 1875.

Since that date fifty-one additional cases have been received—48 males, 3 females—making a total of fifty-two cases to account for. The hospital was designed for the treatment of small-pox exclusively, and the leprosic have been admitted only when found necessary to relieve other public institutions, or perhaps the streets of their presence.

How Disposed of.—Three invoices, aggregating forty-five (45), have been sent by the authorities to China. Three (3) have died of the disease. One (1) from accidental causes. One (1) escaped. One (1) committed suicide, and one (1), a Caucasian, is still an inmate.

Ages of the Patients.

Between 15 and 20.....	4
Between 20 and 25.....	9
Between 25 and 30.....	17
Between 30 and 40.....	13
Between 40 and 50.....	9

No record has been kept of the duration of the disease, at the date of admission, nor of the length of time the patient had been in the country—omissions much to be regretted, and doubtless due to the fact that their detention here was but temporary, and pending shipment. It is not assumption to say they were to appearances healthy when they entered the port, the Chinese Six Companies being too keenly awake to their own interest to import unproductive labor, but that they were the subjects of the disease in its incubative stage at the date of shipment is an inference irresistible.

Type of Disease.

Anesthetic	14
Tubercular	9
Mixed	5
Not Given	24

Accompanying these notes you will find a photographic group of lepers, fourteen in number. They comprise the second shipment made to China, June the 2d, 1880. You will also find two cabinet scenes from the same group—fine specimens of the tubercular type of the disease just prior to the commencement of the ulcerative process.

Duration of the Disease.—From personal observation I can give no information on this point, having seen but one fatal case. In March, 1876, when I joined the hospital, it contained a young Chinaman then convalescent from varioloid. His face was covered with small tubercles (non-ulcerative), usually seen in the convalescent stage of that disease, but they did not become absorbed. He was discharged in April, but not till the similarity of these tubercles to early leprosy had been the subject of comment. On the 28th of July, 1879, he was re-admitted with tuberculous leprosy fully developed, and he died of phthisis on the 20th of the following March, four years from the first appearance of the disease. For the two years next succeeding his discharge from the hospital in 1876, he had been laboriously employed as a miner.

Per contra *Fo Lim* and *To Gan* were both inmates of the hospital two years or more; were both the subjects of the disease several years prior to admission, and were in better physical condition when discharged than when admitted. The former had been eight years in the country and four or five years the subject of the disease. Ulceration had commenced in the fossa between the lower lip and prominence of the chin, but under a more generous diet than he had been accustomed to, it soon healed, and his general health continued good during the period of his stay in the hospital.

For the past two years alimentation has been our only treatment. The leper requires good nutrition and wants it often. The ration of the American army is insufficient for his support until the last stage of the disease has been reached.

General Prevalence.—Any opinion as to the number of these unfortunates now in the city would be mere conjecture. I have seen none out of the Chinese quarter, and without police aid it would be useless to look for it there. It is less general than is commonly believed, yet the fact is a significant one, that, on the 2d of June, every known leper in the city was shipped for China, and before the end of the year fourteen new cases have accumulated upon our hands. The Chinese Consul claims hospital rights for these people; the authorities resist the claim, and the courts are now adjudicating upon the question.

Is it Contagious?—English writers in India say it is not, or at least not in the sense in which the term is usually employed; but the Chinaman who has been familiar with the disease and its traditions for centuries, avoids the leper with great care, and will stand for hours rather than occupy a seat that has been vacated by one.

Heredity.—The leper maintains the most profound silence upon this subject. I have seen but one who admitted its existence as a family taint, and he would, it is probable, have denied it, had not the inquiry been made prior to a conference with his fellows in the lazarette.

Incubation.—*Ah Yong* had been five years, *Ah Tong* three years, and *Lee Fong* (in whom the disease had but recently declared itself) five years in the country before any symptom appeared. *Tu Wuog*, for several years a marine fireman in the employ of Pacific Mail Co., between this port and the Orient, a man of fine physique, and well nourished, developed the disease

at 32, after five years of generous diet on board an American steamship, and surrounded by healthful sanitary conditions. *Thos. Stanton*, present age 47, after a residence of nine years in Madras, Ceylon and Bombay, sailed from Calcutta for New York, December 24, 1870. For three and a half years after his arrival he remained in good health and worked as stone-mason's assistant in Cleveland, Nashville, and other western cities. In the autumn of 1874, becoming the subject of rheumatic pains, he concluded to seek a milder climate, and going to Louisiana he remained there and in other of the cotton States till the spring of 1878. During his stay in the South he had worked a portion of the time, "but did not feel well at all;" and, although he had no medical advice, he "knew it must be that his blood was out of order, or these pimples wouldn't have come out on his face" (leprosic tubercles). Concluding to work his way back to Bombay, he reached Sacramento during the vintage of '78, and obtained employment in the drying-room of a raisin factory. He was the only white man in the room. During the second day his fellow-workmen discontinued their work and declined to resume until the foreman was discharged. The Chinamen had recognized the disease—more successful in this particular than one of the visiting surgeons of a hospital, who, for a period of six months subsequent, treated him as a syphilitic. He was admitted to this hospital March 4, 1880, with fully developed leprosy of the mixed variety.

Six months since his appetite failed him. The numbness in the toes, of which he had formerly complained, gradually extended up the feet: his voice became husky, cough troublesome; the muscles are wasting, the toes sloughing; he has become very irritable, and he will, it is probable, succumb to the disease during the current year. In this instance, the only one in which I have been able to obtain a detailed history, the earliest symptom of the disease made its appearance four years after leaving the locality where it is assumed to have been acquired.

Symptoms.—No better description need be looked for than that given by Dr. Tilbury Fox, on pages 312 and 313 of his able work on the skin. Of the functional lesion in the parts supplied by the ulnar nerve, I will state that in the group of fourteen I send you it was strongly marked in nine, and a very

good idea of the changes which followed can be obtained by examining them (slightly magnified) in the two arm views.

Before closing, I desire to express my regret that I am not able to procure for you photographs of the cabinet size, but I find they are out of print and the negatives lost or rendered useless.

I am, my dear Doctor, respectfully your obd't servant,

JOHN W. FOYE, M.D.

In *Oregon*, also, the disease has appeared among the Chinese immigrants, steps having been taken in February last by the authorities to remove five lepers from the Portland poor-farm and to re-ship them to China.

DR. CHARLES HEITZMANN, of New York, read a paper entitled
Clinical Experience in the Use of the Solution of Oxysulphuret of Calcium.

This solution, commonly called after the name of Dr. Vlemingcx, who first introduced its use, has been long known in dermatology. It has been used chiefly for the treatment of scabies, and by Hebra for the removal of patches of psoriasis.

The solution of oxysulphuret of calcium is prepared as follows: Take one part of quicklime, two parts of precipitated sulphur, and twenty parts of water; boil in a china or glass vessel to the remnant of twelve parts, and filter. The dark-brown liquid resultant has a disagreeable smell and is a strong caustic. Oil of anise will in part obviate the objectionable smell. The solution may be diluted with water or alcohol, the latter precipitating the sulphur, but not materially altering its composition.

The writer had used it in thirty cases of psoriasis of the body, the solution being applied in full strength or diluted, according to the susceptibility of the patient's skin, being rubbed into the patches with a piece of flannel until slight stinging ensues. Care must be taken not to irritate the skin by a too strong solution. Tar ointment is afterward to be rubbed into the patches.

The writer had employed the solution of oxysulphuret of calcium in ninety-five cases of acne disseminata, of which seventy-five were cured; in twelve the result was unknown, and in three cases the disease was aggravated. The solution is in these cases to be diluted with three to six parts of water, rubbed into the face with a flannel rag, and left on over night.

The next morning the face is washed with soap and water. The solution may be employed of greater strength as the patient becomes accustomed to it. Cold-cream may be used to disguise the slight desquamation.

The writer had also employed the solution in a few cases of chronic eczema, fourteen in all. In four cases of chronic eczema of the scrotum, the application of dilute Vlemingckx's solution, gradually strengthened, resulted in a cure.

Thirty-six cases of rosacea were treated by the writer, with good result in twenty-nine cases.

A limited number of cases of tinea tonsurans were treated by means of the solution, but without beneficial result. Eleven cases of tinea versicolor were treated, with brilliant success in every instance, the solution being used in half strength. It appears to be the safest and surest remedy in this affection.

Finally, six cases of scabies were treated, all successfully.

DR. F. B. GREENOUGH, of Boston, asked the writer if the solution was to be applied over the whole face in acne, or to the lesions only. Dr. Heitzmann said it was to be applied over the whole face.

DR. WHITE, of Boston, said he had formerly used Vlemingckx's solution a great deal, but of late years had used it but little. Patients object to the remedy, and it is difficult to get them to use it long enough to do good. He was at present accustomed to use other preparations of sulphur which are less objectionable. Vlemingckx's solution is apt to produce excessive action when used strong enough to be beneficial. He had never used it in acne, and thought it doubtful if he ever should. His patients, mostly young ladies would, he was quite sure, object to the employment of so disagreeable a remedy, and moreover metallic articles in the neighborhood are apt to be affected by the fumes, which makes it very objectionable. He would, however, agree with Dr. Heitzmann as to its value in acne. As regarded the use of sulphur preparations in the treatment of pigment anomalies (hypertrophy), Dr. White did not consider them efficacious; much less so, certainly, than other remedies.

DR. HEITZMANN said, in reply to Dr. White, that thin-skinned persons did not, in his experience, object to the use of Vlemingckx's solution, and inquired what alkaline sulphur remedy Dr. White used.

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DR. WHITE said that he sometimes used sulphur and *sapo viridis*.

DR. HEITZMANN said he had tried sulphuret of potassium, but had not obtained good results.

DR. WIGGLESWORTH, of Boston, said that the mechanical, as well as the medicinal, treatment of skin diseases should always be kept in mind as of importance. He expressed his preference for sulphur ointments as bland and soothing. He was also accustomed to use on occasions carbonate of potassium and sulphur.

DR. DUHRING, of Philadelphia, inquired of Dr. Heitzmann regarding the amount of scaling in cases of acne treated by Vlemingckx's solution, and asked if he followed it up with bland ointments.

DR. HEITZMANN replied that the amount of scaling depended very much on the character of the given case. He always began with a diluted solution, gradually increasing its strength.

DR. HEITZMANN then read a paper entitled

Remarks on Akido-galvano-cautery (Electrolysis) for Epilation.

Referring to a paper by himself, giving experiments in the injection of caustic liquids for the removal of superfluous hairs, and read at a previous meeting of the Association, the writer said that these experiments had resulted unsatisfactorily in the case reported. The lady had since then consulted prominent Viennese dermatologists, who assured her that no cure could be effected in her case, and that any one who should promise such a cure would be a charlatan.

The discussion on that paper by members of the Association present at the time, had re-drawn the writer's attention to the method termed by its inventors. Drs. Hardaway and Michel, of St. Louis, "Electrolysis," and he had employed it in the case referred to. The term "electrolysis" is objectionable, as it involves the idea of an electric action upon the tissues without thermic influence. The existence of such an action has, however, as yet not been proved. What we use for the destruction of the hair bulb is, in reality, the galvano-cautery. The action being induced by a delicate needle, the writer suggested prefixing the word "Akis," and designating the method "Akido-galvano-cautery," or "Akido-cautery." The credit of the invention of the method belongs to Dr. Michel, of St. Louis, and it was first

applied to the destruction of the hairs of the face by Dr. Hardaway, of St. Louis (in a paper read before this Association in 1878). Dr. George Fox deserves credit for certain improvements in the method.

Dr. Heitzmann, the writer, uses the "chloride of silver" battery of the "Electro-medical Company," of Union Square, New York, consisting of twenty cells in a perfectly closed case, neat-looking, easily transportable, and needing no filling for one to two years. He finds five or six cells sufficient, and the operation successful and almost painless; several minutes, however, being required to destroy each hair bulb. The negative pole, armed with a dentist's brooch, is applied to the bottom of the hair pouch, the positive pole, armed with a wet sponge, being held in the patient's hand. Very soon after the current is closed, a whitish discoloration is noticed around the needle, then flakes of epidermis become detached from the mouth of the hair pouch, a little frothing appears, with marked redness about the needle. Repeated gentle trials with the tweezers finally succeed in detaching the hair without the least force, the root of the extracted hair looking as if burned through, and slightly discolored. The hair pouch is gaping, and a second insertion of the needle into the pouch for a few moments is an additional security for the complete eradication of stronger hairs. A wheal appears immediately after the operation, and a small papula or pustule on the succeeding days, which soon disappears without leaving a noteworthy disfiguration.

Some hairs return again. These are usually sufficiently large within three weeks to justify a second operation.

During the winter months of 1880-81 Dr. Heitzmann removed 450 hairs from the patient operated upon, leaving only downy hairs behind. Three months later only six of these had again reappeared. The result of this new American method is brilliant indeed, and honor is due to our brethren of the Dermatological Association who have initiated and perfected the procedure.

DR. WHITE, of Boston, questioned the accuracy of Dr. Heitzmann's statement that the influence of the cautery is purely thermal. He thought it electrolytic. He (Dr. White) had operated by the method described in ten cases during the past year. He had not succeeded so well in his earlier cases as in the more recent ones. He was accustomed to employ a stronger current than Dr. Heitzmann had used—fifteen cells. He

usually placed the positive pole as near as possible to the needle, and kept it under his own control. With all precautions, many cases must be gone over again after three weeks. Dr. White said that, in his experience, scars are usually left; he thought that usually essential to the cure. In some cases, however, he did not find a scar.

DR. DUHRING, of Philadelphia, said, in reference to Dr. White's opinion with regard to the action of the cautery, that he thought it both thermic and electrolytic. He had seen the needle brought to a red heat. He had, after a prolonged trial, abandoned the use of the dental nerve brooch, and now used a fine cambric needle, ground down by a skillful workman. He had devised a light and convenient instrument, made by Fleming, of Philadelphia, for holding the needle, and which he had described and figured in the July number of the *American Journal of the Medical Sciences*. He thought that the operation demanded peculiar manual skill, and considered himself as succeeding at present much better than formerly. Sometimes he found scarring to result, and sometimes not.

DR. WHITE said he never employed thermal influence. His needle was always cold to the touch. He had had no trouble with the dental brooch, which was flexible and elastic. He asked if Dr. Duhring's brooches had been of foreign manufacture.

DR. DUHRING said they had been of foreign make. He inquired if Dr. White did not find pain caused by applying the positive and negative poles so near each other.

DR. WHITE said that he had not found pain result from the procedure mentioned. He had used the chloride of silver battery employed by Dr. Heitzmann, and was well pleased with its performance. He inquired if any of the members had observed the growth of new hairs as a result of the application of the positive pole to the skin.

None of the members had observed this phenomenon.

REPORT OF THE COMMITTEE ON NOMENCLATURE AND CLASSIFICATION

The Committee on Nomenclature and Classification reported that no changes were suggested. On motion the report was accepted.

THE REPORT OF THE SPECIAL COMMITTEE ON MICROSCOPIC
EXAMINATION OF A SPECIMEN OF AINHUM

was then read by the Chairman, DR. HEITZMANN, of New York, as follows :

The specimen submitted to your committee consisted of the small toe of a negro from Brazil, which by constriction on the level of the fold between the toe and the foot, had been separated from the body. It represents an ovoid mass, about two inches in longitudinal, and one and a half inches in transverse diameter, of a dark brown color, the surface being nodulated, and especially on the distal end supplied with distinct rows of papillæ. On the dorsal surface of the distal end there is a well-preserved nail; in the midst of the plantar surface there is a roundish field, one quarter inch in diameter, uncolored and uneven, the place where the pedicle was located.

Frontal and sagittal sections made through this mass, to the naked eye exhibit a very broad epithelial investment all around, except on the place of the former pedicle. The epithelial investment consists of an outer, dark brown, and an inner, colorless layer, the boundary line between these two portions being highly scalloped. The papillary layer is enlarged to the diameter of two millimeters, and the papillæ are so much elongated as to be plainly perceptible to the naked eye.

The main bulk of the toe is composed of coarse bundles of connective tissue in a reticular arrangement, and the meshes inclosed by the bundles hold some fat. The layer next to the bone, evidently the thickened periosteum, is produced by a very dense fibrous tissue, without reticular structure. On the frontal section the phalanges, of which only the second and the third are imbedded in the mass of the toe, look entirely displaced, so as to assume a direction outward and upward, in an acute angle of nearly 45° . The diameters of the phalanges are not noticeably changed. On the outer surface of the phalanges there is a broad layer of tendinous tissue attached to the periosteum. This tendon occupies the central portion of the place of detachment, where it terminates in a rounded-off extremity, a little over one millimeter in width. Around the end of the tendon both the derma and the epithelial layers terminate with sloping ends, which slightly protrude around the tendon.

Sections made in a vertical direction with the razor, under the

microscope exhibited a considerable widening of the epithelial layers, which consist of several strata of alternating horny and protoplasmic epithelia. In the horny portion there are scattered fat-globules in moderate quantity. The papillæ are very much elongated, though not noticeably widened in their transverse diameter except on their bases. Horizontal sections give beautiful figures of the transversely-cut papillæ, and the very broad, concentrically arranged layers of the epithelia.

The derma, which, as seen by the naked eye, is the formation largely prevailing, consists of coarse bundles of fibrous connective tissue, in a very irregular arrangement. The meshes left between the bundles hold either fat-globules or a myxomatous connective tissue, crowded with medullary or inflammatory corpuscles. Such corpuscles are visible throughout the fibrous portion also, but here in a much smaller number. Scanty remnants of the coils of the sudoriparous glands are visible, and also ducts of such glands. The arteries traversing the derma are surrounded by a very distinct, as a rule, broadened layer of smooth muscles, while their calibre, on an average, is narrowed. The veins, on the contrary, are without exception considerably enlarged and completely filled with blood. The periosteum ensheathing the phalanges consists of a dense fibrous connective tissue, whose bundles are rectangularly interlaced and hold a relatively small number of inflammatory corpuscles. The portion of the periosteum nearest to the bone is to a great extent myxomatous in nature, and in some places crowded with inflammatory corpuscles.

The surface of the bone, and also the trabeculæ of the cancellous portion, are irregularly jagged, supplied with lesions and bay-like excavations, which either are filled with a fully developed connective tissue, or with masses of inflammatory corpuscles. The picture reminds of the cementum of deciduous teeth, whose surface, at the time when the tooth becomes reduced, melted down, looks in a very similar way. The medullary spaces of the cancellous bony tissue are filled either with fat-globules, or with a myxomatous connective tissue, the veins being also dilated and engorged with blood-corpuscles.

The articular cartilage on the lower extremity of the second phalanx is considerably reduced in size. Its surface on many places looks crowded like that of the bone, and the bay-like

excavations are filled either with inflammatory corpuscles or a fully developed connective tissue.

Lastly, it is worth mentioning, that on some places in the fat-tissue the fat-globules are separated from each other by large spaces, filled with a finely granular mass, namely, coagulated albumen. This image corresponds to that of œdema.

The result of the researches of your committee is, that the specimen of ainhum fully answers the process that we are accustomed to call hypertrophy or hyperplasia of the tissues due to a chronic inflammatory process. The hyperplasia involves both the epithelial investment of the skin and the derma, while the bony and cartilaginous tissue, due to the slow inflammation, is gradually reduced into fibrous connective tissue. As to the cause of this marked hyperplasia of the toe, your committee submit the following suggestions :

The description of the process by Drs. Da Silva Lima and J. Nevins Hyde, draws attention to a hard constricting ring on the plantar surface of the foot in the fold, which demarcates the toe from the sole of the foot. This fold strictly corresponds to the articulation of the first with the second phalanx of the toe, and a gradual constriction of the tissues necessarily will lead to a separation in the named articulation. To what is this constriction due? Is it an inflammatory process in the derma, or is it an atrophic process in the derma, kindred to that of lepra mutilans? Nothing of the kind is indicated by the specimen. The derma noticeably slopes in the immediate vicinity of the strangulated place, whose middle portion, as mentioned above, is occupied by the flexor tendon ; there are no signs of an inflammatory thickening on the place of the constriction.

Furthermore, how could a primary constriction of the derma explain the remarkable deviation of the two phalanges within the stump? It was said before that the phalanges stand in an acute angle outward and upward. Can a narrow constricting ring ever produce such a deviation of bony formations?

The probability, in fact, is that the constricting ring is produced artificially, by a thin ligature tied around the toe, if not continually, at least for certain periods. The deviation of the phalanges, and the changes within the tissues, indicate a self-mutilation rather, done with a purpose and persistence, than a disease of a specific nature.

C. HEITZMANN, I. E. ATKINSON, *Members of the Committee.*

The report of the committee was on motion accepted.

Exhibition of Specimens.

Various microscopic specimens, illustrating the several papers read at the meeting, were then placed under several microscopes and demonstrated to the members of the Association. After considerable discussion on these, the newly-elected officers of the Association for the year 1881-2 were duly installed, and the Association then adjourned, to meet in Newport, R. I., on September 1, 2 and 3, 1882.

NERVOUSNESS RESULTING FROM INTEMPERANCE.—We have found Celerina exceedingly valuable in the treatment of nervous headache, nervous exhaustion, and other associated ailments of women : but the cases to which we now desire to call attention, where the Celerina is of inestimable value, are those suffering from nervousness resulting from intemperance. Every practitioner meets with such cases. Men, and sometimes women, come to us trembling and apparently exhausted, all from the effects of intemperance. Such cases are approaching delirium tremens. Celerina is the most appropriate prescription we can give them. A few doses of bromide of potassium may be given, alternated with the Celerina, at first ; but after this, for permanent effects, we depend upon the Celerina.—*American Med. Journal.*

MAYOR LATROBE, of Baltimore, has signed an ordinance absolutely prohibiting the sale of toy pistols in that city. The large number of deaths by lockjaw last summer prompted the passing of the act.—*Boston Med. and Surg. Journal.*

RESECTION OF SMALL INTESTINE.—Kœberli has reported to the French Academy cases of resection of the small intestine, and advocates this operation. This is not new ; the operation was successfully performed at Manassas Junction, Va., during the war.—*Medical News.*

Foreign Correspondence.

LONDON, Nov. 6, 1881.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—

The medical visitor in London is impressed during his progress through the city in almost any direction by the immense numbers and diverse purposes of the hospitals. One would infer that almost every cubic inch of the human form divine was the object of special study and treatment. The chest, throat, eye, ear, genito-urinary tract, rectum, brain, teeth, skin, hip-joint, have each one or more hospitals professing to treat the special diseases to which the organ in question is subject; beside which children, women pregnant and women not pregnant, consumptives, cancerous patients, the paralyzed and epileptic, the insane and those suspected of a clandestine tendency toward insanity, the syphilitic, the fever patients, those requiring orthopædic treatment, are the objects of tender solicitude to hospitals treating these various ailments respectively. Besides these are the numerous general hospitals, and almost innumerable dispensaries, public and private.

Yet all these various institutions have, with the exception of a few well-endowed hospitals such as Guy's and St. Bartholomew's, one phase in common, namely, that they are "supported by voluntary contributions," a fact published in enormous letters in a most conspicuous place on the building. A series of boxes placed at salient points and adorned with scriptural quotations, with touching allusions to your deceased aunt, explains itself. Yet most of these institutions are more or less endowed, and not strictly dependent upon the benevolence of the public.

Another more agreeable feature is also common to all. With

rare exception, the simple presentation of one's professional card secures him the courteous attention of the physician in attendance, and a cordial invitation, at the conclusion of the visit, to come again.

The natural inference, from the sight of these hospitals, that the London profession is an army of specialists, is but half correct. It is true that most of the better men study especially a particular organ or system of organs, but not to the exclusion of the rest. The London ophthalmologist is a general surgeon, the London neurologist is a general physician. Hence it happens that the specialist often knows surprisingly little of his own "particular vanity," while the general physician or surgeon seems astonishingly familiar with various specialties. Yet no one, with rare exceptions, attempts to cover more than half the medical field—medicine proper, or surgery. The line of demarcation between physician and surgeon is sharply drawn in professional qualifications as well as in practice. The physician is supposed to have studied longer, and to have passed more trying ordeals of examination; he affects somewhat contemptuous ignorance of matters surgical, and calls in a surgeon even to amputate a finger. The surgeon, on the other hand, while appropriating everything requiring manual interference, and doing "light jobs" on the viscera, refers with reverent humility all the more important internal lesions to the supposed superior wisdom of the physician. Nor is this distinction merely formal, as I was at first inclined to suppose. I once heard Mr. Hutchinson's assistant surgeon, in narrating Mr. H.'s endeavors to elucidate the relations between certain skin diseases and systemic conditions, say, by way of apology for lack of success, "Mr. Hutchinson is, however, only a surgeon." The man whose work is familiar to all is supposed to know nothing more about the body than the probe, scalpel and ophthalmoscope reveal; and has no other conventional title than if he were a successful brewer or politician!

That Jonathan Hutchinson is so ignorant of medicine I do not believe; yet a surgeon of a large hospital, upon being consulted the other day as to the (possibly aneurismal) nature of a tumor in the thigh, called for a stethoscope; a binaural instrument was offered him; he hesitated, then, with the firmness of despair,

grasped it manfully, applied it to his ears, point backward, and after listening (?) a moment, gravely, and doubtless truthfully announced, that he heard nothing. An incision revealed the wall of an aneurism about one-half inch below the skin.

The medical teaching here presents great advantages combined with glaring deficiencies. I will abstain from discussion, and content myself with remarking that while the student has here opportunities for clinical observation unequaled at any school, American or Continental, of which I have personal knowledge, there is, nevertheless, such imperfect individual instruction in methods of diagnosis as to permit a man to remain quite unacquainted with microscope, ophthalmoscope, laryngoscope, and even stethoscope. The students in a certain large hospital here call all adventitious sounds in the lungs "crepitations." A careful study as to what a "crepitation" was, revealed to me, under that name, sometimes the moist râles of bronchitis, sometimes the dry râles of asthma, again the crepitant râle of incipient pneumonia, but especially the gurgling râles of advanced phthisis. Whether any distinction is made as to the pathological significance of these various "crepitations," I am unable to say.

A word as to hospital management, in discussing which I shall refer to the London Hospital—the largest though not the wealthiest in Great Britain—where I have for four months been clinical assistant.

Aside from the usual amount of red tape essential to the average Briton's ideas of propriety here and hereafter, the management is generally excellent. The board of managers or "governors" (this word is doubtless chosen to imply, not a judicial, but a paternal relation to the hospital, since it is young Britannia's pet name for his most immediate male ancestor), is less obnoxious than such bodies usually are. The domestic affairs are in the hands of non-professional men, but are probably as good as possible under these circumstances. But it is of the ward work that I wish especially to speak. Each ward, containing ordinarily about forty-five beds, is furnished with a head-nurse, technically called "sister," and two assistants—all of them of better social grade than the usual American or Continental nurse; indeed it is not uncommon to see ladies (in the English sense) performing

these onerous duties. These three are replaced at night by a fourth nurse. The sister is responsible for the details of the nursing, and performs, personally, the less disagreeable work. Every medical ward is provided with drugs required for emergencies ; a set of tubes, reagents, etc., for urinary examinations ; a laryngoscope, stethoscope, hypodermic syringe. . The surgical wards have each a small operating case, air-cushion dressings, etc. Ventilation is by flues, and dubious. Iron bedsteads with hair-mattresses are used, the bed provided with half curtains, which secure to the patient a certain degree of privacy, at least above the waist. Over each bed hang two written records—one containing directions for the nurses—medicines, applications, etc.; the other a history of the case ; a chart of the temperature, taken twice daily ; the quantity of urine passed in ounces ; the number of stools (all recorded by the nurses), and the condition of the urine, recorded once a week by the clinical clerk. Diagrams are provided, showing in outline the various regions of the chest and abdomen, for convenience and accuracy in recording the location of tumors, lesions, etc. ; in special cases charts for electrical reactions, elaborate analyses of urine in diabetes, appearances of the fundus oculi in cranial disease, etc., are furnished.

In a wire frame hang cards naming the articles which compose the patient's diet. Stimulants—*i. e.*, distilled—are used only in special cases, but beer is included among dietary articles.

The floors are plain wood, a broad matting extending down the center of the ward. A comfortable fire on a large hearth relieves the otherwise desolate aspect. The mental and moral needs are supplied by a profusion of pious texts distributed around the walls ; by prayers from the Episcopal service at each bed, and by a few dozen books.

Children are accommodated in four large wards ; and two others are devoted to Hebrew patients. No maternity nor contagious fever cases are admitted.

The points of interest in the practice of this and other hospitals will be referred to later.

W. T. BELFIELD.

Domestic Correspondence.

ARTICLE X.

EDITORS CHICAGO MEDICAL JOURNAL AND EXAMINER:—

No one will dispute the assertion that there cannot be found a more enterprising and progressive city than Chicago; and it is therefore to me a matter of pleasure as well as pride that I am able to communicate to you and your readers a new venture, which cannot fail to add to the fame of this beautiful city. Recognizing the fact that the most advanced stage of civilization is inconsistent with a limited number of physicians and few professors of medicine, a number of gentlemen prominent in the profession in this city have taken the necessary steps to incorporate a new college, to be known as the Illinois State University of Medicine, to be located, of course, in Chicago. It is true that a similar institution, called the Chicago College of Physicians and Surgeons, is in process of organization, in order to meet "a want long felt," but the University of Medicine presents features so entirely distinct from the former, that they ought not to be confounded by any person possessed of average intelligence.

As is observed from the advertisements for professors in the various medical journals, the number of professorships in the College of Physicians and Surgeons will not exceed twenty, and the resident of any State in the Union is eligible for a professorship, if he possesses the necessary qualifications, which may be ascertained by addressing the Secretary (in confidence). Not so, however, in the Illinois State University of Medicine. In conversation with the accomplished Dean and other members of the prospective faculty, I learn that no one shall be eligible for a professorship unless he be a citizen of the State of Illinois, the incorporators being well aware that there is no need to go outside

the State, or this city even, to secure abundant material for professorships.

Again, it is the determination of the incorporators to greatly enlarge the miserably small percentage of physicians who are professors, and by every means in their power to contribute to the very essence of medical progress by so enlarging the number of professorships in the university, that the percentage of physicians in this city, at least, who are not professors, will be very small indeed. To accomplish this laudable object, the number of professorships in the university will be unlimited, new chairs to be constantly created as additional candidates may appear. Such neglected branches of medicine and general science as the diseases of the umbilicus, ante-natal impressions, medical astrology, etc., will be duly represented by separate and distinct chairs.

I am also informed by the Dean that all graduates who are residents of this State will be presented with a lectureship in the summer course of the university, and the degree of Master of Arts. Many other features of interest which will distinguish the university might be mentioned, but as the prospectus will shortly be issued, I forbear to trespass any further on your limited space.

With the Illinois State University of Medicine, the Chicago College of Physicians and Surgeons, the Chicago Medical College, the Rush Medical College, the Woman's Medical College, the Hahnemann Medical College, the Chicago Homœopathic College and the Bennett Eclectic Medical College, all located in Chicago, we may indeed pride ourselves that we not only lead the United States, but all creation, in the matter of medical education, and assure ourselves that the time is not far distant which will truly be the millenium, when every citizen of Illinois will be a Doctor of Medicine, and every practitioner of the noble art a professor as well.

CHICAGO, ILL.

E. J. DOERING.

ARTICLE XI.

PRESCOTT, WIS., Sept. 29, 1881.

MESSRS. EDITORS:—I have recently had a case in which from personal considerations I was deeply interested, but it also possessed considerable professional interest to me, not having in thirty years' practice met a similar one, and also from the incertitude attending the diagnosis.

There are one or two points in the case on which I would like your opinion, and therefore inclose notes of the case. I shall be pleased to hear from you at your leisure.

Mrs H., aged twenty-six years, of highly nervous organization, general health rather feeble, has been married five years, and has suffered severely from dysmenorrhoea during that time, excepting last few periods. Supposed to be three months advanced in her first pregnancy. Soon after sitting on the damp ground, was attacked suddenly with severe pain in left side, near upper edge of iliac fossa, radiating in direction of distribution of upper lumbar nerves. There was considerable tenderness, with apparently an abnormal fullness in the region indicated, though no clearly defined tumor could be made out, the abdomen being well covered by adipose tissue. The pain was soon relieved by anodyne and warm applications. After the action of a cathartic, the fullness nearly but not entirely disappeared. During the next three weeks she had several similar attacks, lasting from a few moments to an hour or two; sometimes two or three in twenty-four hours, and then not for two or three days. At first there was no vesical irritation, but subsequently there was considerable vesical tenesmus present during the attacks of pain, which thus had the appearance of ordinary attacks of nephritic colic. The tenderness persisted, but was a source of but little annoyance. Finding that the attacks were much more apt to recur when on her feet, she remained most of the time in a recumbent position.

About three weeks after the first attack, Monday, four A. M., while asleep, she was taken with the same pain, but in an intensely aggravated form. I saw her within a few minutes, and found her screaming with agony. My first impression was that a

rupture into the peritoneum of some abnormal growth—possibly a tubal pregnancy—had occurred; but the persistence of the pain, the severe vesical tenesmus present, and the absence of evidence of shock, except so much as might be fairly attributable to the pain, led me to conclude that a large and rough calculus was attempting the passage of the ureter. (Having recently, in my own person, had this experience, I had a vivid conception of the sufficiency of this cause for all the symptoms). She was as speedily as possible put under the influence of morphine, assisted at first by chloroform inhalations, this influence being kept up during the future of the case. The pain soon became bearable, but did not cease, as if the calculus had passed into the bladder, and I inferred that if a calculus, it had lodged in the ureter. Within thirty-six hours symptoms of severe general peritonitis had supervened, which, despite the treatment adopted, proved fatal Saturday eve, seven days from attack.

An autopsy (which unfortunately had to be conducted hastily) was held, which revealed the anticipated evidences of the fatal peritonitis. In the left iliac fossa was found the *origo mali*—a large, firm, round coagulum, including in its structure the ovary, Fallopian tube and broad ligament. The tumor was easily lifted from its bed, and was found to be at one point in contact with and attached to the left cornua of the uterus by a narrow, short pedicle, consisting of the ovarian ligament and Fallopian tube, twisted so as to have performed a complete revolution on itself. The broad ligament had within it two cysts filled with bloody serum, the larger the size of a large orange, the smaller half that size. The structures of the ovary, tube and broad ligament were so completely infiltrated with coagulated effused blood, that the whole mass of the tumor had the appearance of a homogeneous clot. Part of this coagulum within these organs, in appearance at least, seemed to antedate the main mass of coagulum. The point from which the hæmorrhage proceeded was not discovered. The tube itself was thought to be intact. Within the uterus was a four months foetus.

The points of the case upon which I would like your opinion are: 1. The cause of the repeated attacks of pain. My own impression is that they were due to repeated small hæmorrhages

which gradually infiltrated and distended the structure of these organs, until finally a rupture into the peritoneal cavity occurred ; though, in this case, I do not see why an anodyne and warmth should have always controlled the pain within a short time.

2. Had the twisting of the pedicle (which I would have thought anatomically impossible without rupture of some of the tissues) anything to do with the attacks of pain or the hæmorrhage?

ARTICLE XII.

TO THE EDITORS OF THE CHICAGO MEDICAL JOURNAL AND EXAMINER :—I have read with pleasure and profit Dr. Clevenger's article on Medical Electricity in the November number of the MEDICAL JOURNAL AND EXAMINER, and will say at once that I am in hearty accord with his criticisms upon inexact, unscientific and pretentious electro-therapeutic measures. This protest against indiscriminate electrization should be entered again and again until no reputable physician would dare to pump, so to speak, electricity into or through his patient with the view of counteracting a suspected, though unlocalized and undiagnosed diseased condition. I have always contended that the conscientious practice of electrization must be founded on the dictum of Benedict, viz : To treat the seat of disease, or, to make an amendment to this rule often found necessary, to treat also the part affected. For instance, in a hemiplegia both cerebrum and the affected nerves and muscles are to be treated. Regarding, then, both an accurate diagnosis and a localized treatment as essential, I can but sympathize with Dr. Clevenger's criticisms upon unscientific methods. The terms "central galvanization," and "general faradization," for instance, appear to me to be as pernicious as unscientific. But, just at this point, and in this connection, I wish to say that I fear Dr. Clevenger has received a wrong impression of the methods pursued by Charcot and Vigouroux in statical electrizations. Referring to a discussion at the meeting of the American Neurological Association held in June last, he writes : "The methods of Charcot and Vigouroux in the

use of a large platform whereon numbers of patients were electrized at once, were characterized as unwarrantable." To be sure, he was justified in this idea by the words of a member * of the Association, who stated that "they" (Charcot and Vigouroux) would huddle together upon a single isolated stool cases of hystero-epilepsy, locomotor ataxia, paralysis agitans, a case of anæsthesia, and a case of headache, thus forming a series of pathological conditions which had nothing in common, and apply the same current to all of them." The term "unwarrantable" should rather be applied to this last quoted statement than to Charcot's methods. The idea conveyed is entirely fallacious. The treatment did not consist in being "huddled" upon a platform to which static electricity was conveyed. This was merely an essential preparatory condition of general "electrification" or "charge." The real treatment consisted in drawing sparks from the seat of disease in each instance where this was practicable—in the ataxic, from over the spine, in paralysis agitans from over the spine and from the shaking members, in the hystero-epileptic, and the anæsthetic from the affected part, and in cases of peripheral paralysis from the nerve involved. Surely this was conscientious and scientific electrization, and scarcely deserves to be called an unwarrantable method; unless, indeed, it is "unwarrantable" to try new remedies, or methods, or forms of electricity; unless, indeed, it is more desirable to confine oneself to what each critic may consider the settled and fixed facts of medicine, and to assert that there is no opportunity for advance in the treatment of disease.

But I do not here intend to re-argue the claims that static electrization has upon the medical profession. These claims I have already set forth in several publications, notably in a paper read before the New York Academy of Medicine, March 3, 1881, and printed in the *New York Medical Record* of April 2, 1881.

In this paper I first led the way to the present resurrection of static electrization in America, following the indications and teachings of Charcot and Vigouroux in France. I am gratified that the subject is receiving a fair investigation.

From the standpoint of the medical electrician I have little

* Dr. Amidon.

interest in the matter, though I think that in the future the specialist in electricity will feel obliged to resort to statical as well as galvanic and faradic electrizations ; but as a physician interested in the neurological side of diseases, I am confident that statical electrization has a place in effective treatment as fixed, understandable and reliable as strychnia, the bromides, the douche, or as have the more familiar galvanization and faradization.

Waiving, then, discussion as to the present status of statical electricity, I ask brief space for comment on a point more nearly personal.

Referring still to the last meeting of the American Neurological Association, Dr. Clevenger remarks : “ Dr. Morton described an apparatus, *by means of which he claimed results similar to those obtained by the secondary coil, and erroneously called it a new induction current.*” It is not probably meant that I called the “ apparatus ” a new “ induction current ; ” but rather that I claimed to have produced results similar to those obtained by the secondary coil, and that I claimed that I produced these results by means of an induction current, and that this induction current was new.

And first in regard to the results :

The results referred to are nerve and muscle reactions, produced from a Holtz machine, and exactly similar to those produced by the ordinary Faradic machines. These results were publicly and practically demonstrated before the New York Academy of Medicine, and they have been admitted by all physicians and electrical experts who have witnessed them.

It is of these reactions that Dr. Bartholow writes : “ The impression made by the electricity is like that of the faradic machine, but is much less painful, and strong muscular contractions are thus induced with greatly less pain than can correspondingly strong contractions of the muscles be obtained by faradism. We have thus added to our resources an immensely useful instrument for the production of these effects hitherto obtained from faradism.”

Dr. Bartholow's observations have all the more force, since they were made independently of my own experiments, and were published only a few weeks later.

That I justly "claimed" to have produced the results in question may, then, be considered as settled.

Next, as to the second point, that I "erroneously" called the current "a new induction current." Now in all friendliness, I ask Dr. Clevenger to substantiate this criticism. I shall be as glad as any one to be set right in the matter. It is a question not without interest to electricians, owing to the wide range of usefulness possessed by induced currents, and this one in particular, in helping to restore paralyzed muscles and degenerated nerves to their normal functions.

I wish to demonstrate to him, respecting as I do his criticism, and the pages of the journal whence it proceeds, first, that the current I described is a true induction current; and second, that it is new. This demonstration, I may add, seems to me to be superfluous to any one who has evoked the current in actual practice from a Holtz machine. A moment's inspection by any one familiar with induced currents would save pages of writing. And I would apply the same remark to my critics* in the American Neurological Association, who seem to have entirely misapprehended the question. I can only hope that Dr. Clevenger has prematurely accepted their criticism as final, without personal investigation and knowledge of what I claim.

To produce the current in question, remove the connecting bar or chain between the two *outer* tin-foil coatings of the two Leyden jars usually attached to a Holtz machine. Connect ordinary conducting wires and wet-sponge electrodes to each outer coating respectively, and finally connect the two *inner* coatings by the discharging rod. As soon as the machine is set in motion, and the Leyden jars are filled, the discharging rods must be drawn out a very small fraction of an inch, and at once a current is felt between the two sponge electrodes, which in its general characteristics cannot be distinguished from the ordinary Faradic current. The experiment succeeds best with a large Holtz machine.

Next, is this an induced current?

It is a "current," according to the recognized definitions of

* Drs. Amidon, Gradle and Birdsall.

the term, for it is an equalization of two potentials along a conductor. Every electric discharge, however brief, is a current; the spark is a brief current. The voltaic, or so-called "continued current" is no more nor less than a succession of brief discharges; in other words, the potentials are continuously kept up to a rapid discharging point. The Faradic, or induced current, in the same manner, consists simply of the alternations of direction in a current set up through a dielectric by a voltaic discharge; *i. e.*, a "make" and "break" in the battery current.

I consider it demonstrated, then, that whatever electrical process takes place along the conducting wires or through the wet-sponge electrodes, while a Holtz machine is arranged and working as above described, is a current. For the unequal potentials exist in the *outer* tinfoli of the Leyden jars, constantly kept at a fixed point of discharge by the working of the machine; while their equalization takes place along the conducting wire, and between the electrodes properly placed. In this current, as in the induced current, the term is loosely applied to a succession of alternating discharges or influences along a conductor.

Next, is this current an induced current?

It is *induced* or created by influence through the dielectric glass, and by the respective positive and negative electricities *inside* the jars.

We will, for instance, set the machine in motion with its discharging rods one-sixteenth of an inch apart. What happens? A spark, *i. e.*, a current in brief form, passes between the positive electricity of the inside of one jar and the negative electricity of the inside of the second jar. At the same instant, an induced current, *i. e.*, a brief equalization of potentials, takes place between the outsides of the same two Leyden jars. If, now, the discharges from the inside of the two jars are very rapid, as between the electrodes of a Holtz machine, the discharges on the outside are also very rapid, but the discharges from the inside, passing through a bad conductor like air, take the form of zigzag lines of so-called sparks; while the discharges from the outside, being led through tolerably good conductors, do not take a spark form (though they may be made to do so), and we thus

have at the sponge electrodes connected to the two respective outsides an "induced current." It is "induced" for the reasons given. It is a current for the reason given. It is rapidly alternating, like any true induced current; and, to carry the analogy further, the discharge between the inside of the jars is no more nor less than the counterpart of the "battery current," and the succession of spark discharges at the usual discharging rods is no more nor less than the ordinary "make" and "break" in the battery current of an ordinary Faradaic machine. In the one case a continued static discharge, or equalization of potentials, takes place; in the other, we have a voltaic or galvanic discharge and equalization of potentials.

Now, the current obtained from any induction coil (ordinary Faradaic batteries) is often termed the Voltaic induced current. I ventured to call, by analogy, the current I have discovered the "static induced." I have yet to be shown that my terminology is not well selected.

Lastly, as to the point: Is this induction current new?

As a current capable of producing muscle and nerve reactions similar to other induction currents, I claim that it is. Its novelty lies not in the simple scientific fact that a discharge of static electricity provokes in a neighboring but untouching conductor an induced discharge or current, but in the fact that this induced discharge or current of static electricity may be so affected by an *interruption* in the inducing discharge as to cause physiological action in nerve and muscle. Uninterrupted, this induced current produces in the human body no appreciable effect—interrupted, it is a serviceable ally in medicine. After many attempts at manufacturing an efficient interrupter, I found that the spark discharge between the discharging rods was the simplest and best.

And medically it is "a new induction current." In spite of the large medical use to which static electricity has been put during more than a hundred years, I am not aware that any one, before my experiments or publication, made use of any similar medical application of static electricity by means of any arrangement of Leyden jars with any form of frictional or inductive electric machine. I may then, in short, say that although an

induction discharge of static electricity was known to exist, no one ever interrupted it and applied it to the human body, and got physiological results. Herein lies its novelty, and herein it is a discovery or invention.

This is not the place to refer to the physiological effects of the new current. I will simply say that it seems to me, and to others who, on investigation, have come to the same conclusion, that the static induced current produces stronger and more diffused muscular contractions, and with less pain, where pain is necessarily produced, than the voltaic induced currents now in use. In many cases of paralysis (of the anterior tibial group of muscles, of the deltoid, in Bell's paralysis, in infantile spinal paralysis, etc., etc.) I have obtained muscular contractions, where I have not been able to do so with a Gaiffe battery, or with the battery manufactured by the New York Galvano-Faradic company.

I am quite ready to admit that I "must experiment and report more fully before the apparatus will win favor." This I expect to do; but in the meantime, I am sure Dr. Clevenger and the JOURNAL AND EXAMINER will appreciate my desire to make clear to them the fundamental facts in the matter, even though it require what I fear is a somewhat lengthy letter, to arrive at the result. I am

Very respectfully yours,

WILLIAM J. MORTON.

15 EAST 45TH ST., NEW YORK, Nov. 26, 1881.

MEN OF PROGRESS.—There is soon to be published a work with this title. The publisher calls upon the physician, who dictates his own life; he next pays for an engraving of himself, and presto becomes great. It is difficult to escape such a fate. —*Medical News*.

Reviews and Book Notices.

ARTICLE XIII.—THE DISEASES OF CHILDREN. By WILLIAM HENRY DAY, M.D. Second Edition. 8vo., cloth, pp. 752; \$5.00. Philadelphia, Presley Blakiston. Chicago: W. T. Keener, 96 Washington Street. 1881.

This is one of the best works on the diseases of childhood. It is complete, well written, entirely practical, embraces the latest views, and is based on the wide experience of Dr. Day in private and hospital practice. The chapters on Hygiene, Debility and Dentition are very well written, and will prove useful to every reader, notwithstanding the opinion of a few critics to the contrary. The chapter on typhoid fever in infants evinces the fact that the disease is quite common, and has just as often been overlooked by the majority of physicians in the past, being treated as infantile diarrhœa or remittent fever, and many of the ablest practitioners to-day will substantiate this statement.

The statement that simple diarrhœa may pass into all the other varieties is very sensible, and so is the author's treatment of those diseases. A variety of useful prescriptions, specially adapted for busy practitioners, are found through the volume, and by themselves at the end; which arrangement is very commendable. The two last chapters, devoted to diseases of the ear and diseases of the skin, although necessarily brief, will prove useful.

Chapter XVII, given to Intestinal Worms, begins thus: "The presence of worms in the intestinal canal is one of the commonest troubles of childhood." Although this statement is also made by some of the best writers of the continent, it does not hold good in this country, for one reason or another. However, this work is well suited for students and practitioners,

although not any more so than some American works on the same subject. On the whole, the profession will be thankful to Dr. Day for this new edition of his valuable book. H.D.V.

ARTICLE XIV.—CYCLOPÆDIA OF THE PRACTICE OF MEDICINE.
 ZIEMSSSEN. Vol. IX. Diseases of the Liver and Portal Vein, Interstitial Pneumonia. Pp. 928; Id. Supplement, pp. 844. William Wood & Co. 1880–1881.

These two volumes, together with the index, form the completion of perhaps the most important effort at unifying the science of medicine. It is a great credit to Germany to have thus gathered in a small compass all the medical genius of a people preëminent in all sciences. No writer on any disease would be pardoned to-day if he had not had some acquaintance with Ziemssen's *Cyclopædia*, yet for reasons which we will presently state, such cyclopædias present many disadvantages. We see one of these in this ninth volume, where the subject of Diseases of the Liver has been stretched out of proportion to all others to fill 836 pages, though otherwise well treated.

The supplement evinces another disadvantage, and several contributions are too deeply stamped with the "busy practitioner,"—resembling somewhat the usual "Society Transactions." However, the contributions to Syphilis, by Prof. J. N. Hyde, of Chicago; to Diseases of the Pleura, by G. M. Garland, M.D., of Boston; to Diseases of the Nose and of the Pharynx, by J. Solis-Cohen, M.D., and a few others, are as remarkable for their value as for their brevity.

The few illustrations accompanying some of the articles are very poor.

The comparative and well deserved success of this work warrants the prediction that for a decade to come, the profession will be flooded with cyclopædias. Indeed, there is the *Cyclopædia of Chemistry*, that of *Surgery*, and others in view. Whatever their intrinsic value, a second edition of these works is almost impossible, on the one hand; and on the other, science has never exhibited before such wonderful changes as in the last thirty years. In the end, individual efforts generally prove the best. H. D. V.

Original Translations.

ARTICLE XV.

THE VARIOUS CONDITIONS UNDER WHICH ALBUMEN IS FOUND IN THE URINE. By ED. ROBIN.

Urine becomes albuminous : 1. In croup or laryngeal diphtheria ; in ascites, or far-advanced abdominal dropsies ; in that emphysema in which much dyspnœa takes place ; in pulmonary phthisis, especially if complicated with pneumonia, and accompanied by much difficulty in the respiration ; in the child-bearing state, after the abdominal circulation is interfered with ; that is, in those diseases in which a notable diminution of combustion (oxydation) follows a difficult respiration.

2. In cyanosis, whatever its cause, and in heart troubles carried to that extent that the patients are in a state of partial asphyxia ; hence in cases in which an obstacle to the circulation, or a vicious conformation of the heart, prevents hæmotosis from being as complete and as rapid as under ordinary circumstances.

3. In eclampsia.

4. In cholera, a disease which diminishes considerably the oxydation of the blood.

5. In idiopathic or in traumatic lesion of nerve-centers, with lowering of temperature, and a notable diminution of combustion.

6. In diabetes, a disease in which, often enough, some primitive and analogous lesion seems to have existed ; in which, besides, a great amount of sugar in the blood impedes the combustion of albuminous matter and hæmotosis ; in which, finally, as Bouchardat observed, the temperature is lowered by one or two degrees in deeply diseased subjects.

7. In that sort of deficiency of nervous fluid which character-

izes the state called curvature of the spine, which is most likely accompanied by a considerable diminution of calorification.

8. In the natural or artificial suppression of cutaneous perspiration, a suppression which sensibly diminishes the absorbing capacity of blood for oxygen, which may bring on a sort of semi-asphyxia, at any rate, a considerable lowering of temperature, which necessarily produces a marked diminution in the latent combustion of the tissues.

Thus urine becomes albuminous from the use of impermeable artificial coverings to the skin; and the same phenomena is not rare in those diseases, as measles, scarlet fever and small-pox, in which the functions of the skin are liable to be greatly interfered with.

From an analogous cause, urine is albuminous as a sequel of an intense cooling of the surface of the body from cold.

Lastly, Bright's disease, in which the urine is always found to be albuminous, is precisely referred to many of the causes just enumerated: heart disease; changes in the liver; a retardation or suspension of circulation in the abdomen, whatever the cause; a sudden impression of intense cold; large imbibition of alcoholics; pulmonary phthisis with dyspnœa, etc.

As a general rule, the urine of common quadrupeds, and that of birds, does not contain albumen. In reptiles, on the contrary, in batrachia, in frogs, at least, so remarkable on account of their low animal heat, some albumen is always passed with the urine (Dumas).

It is not quite demonstrated, yet very probable, that urine becomes albuminous under the influence of those agents which, according to one, protect to quite an extent from latent combustion. I have but few facts to substantiate that view.

Urine becomes albuminous in protracted anæsthesia, and in slow poisoning by cyanhydric acid; habitual drunkenness predisposes to albuminuria to quite an extent. This state suddenly appears sometimes in syphilitic patients under a course of mercury. . . . The albumen discharged represents so much organic matter which has escaped a transformation into urea and uric acid, from a lack of activity in the oxydation of the blood.

Whatever the true theory, it remains a constant fact that *any important alteration in hæmatosis gives rise to albuminuria*.

The foregoing were condensed from a communication made in 1851 to the *Académie des Sciences*, by myself. The following are facts generally ascertained since :

The fœtus of quadrupeds resembles cold-blooded animals as to its hæmatosis, and its urine is albuminous, and remarkable for a total absence of urea.

When death is approaching, the respiratory movements and the cardiac contractions weaken to quite an extent ; the lungs become congested ; in a word, hæmatosis is considerably diminished, and, as observations prove, this state is liable to cause albuminuria. . . . Septicæmia, hospital gangrene, phlegmonous erysipelas, yellow fever, typhoid, and typhus fever, which are all remarkable for a diminution of hæmatosis, are also remarkable for the frequency of albuminous urine. This condition of the urine is, indeed, one of the most constant symptoms of typhoid fever, and, according to Finger's observation, the renal tissue may remain intact.

From the same cause, miliary sweat, in which hæmatosis is so diminished and the blood so thin, is one of the diseases accompanied by albuminuria.

Jaundice often depends on a rapid diminution of hæmatosis, and it is sometimes accompanied by albuminous urine.

Chlorosis, a type of pathological anæmia, diminishes notably the number of red corpuscles, and often has albuminuria for one of its symptoms.

Gout ; red gravel ; rheumatism ; sicknesses in which hæmatosis is frequently diminished, are often accompanied by albuminuria.

Let the alteration result from tuberculosis, syphilis, cancer, glanders, malaria, scrofula, or other causes, all cachexias have this in common, that they diminish the formation of blood corpuscles, and diminish in a marked degree respiratory combustion. All of them diminish the tenacity of tissues, and end by allowing an escape of albumen with the urine.

In meningitis, the nature and the seat of the disease ; the somnolence, coma, nausea, vomiting, convulsions ; all seem to

indicate marked disturbance in hæmatosis. (See Book III of my works on the Reformation of Medical Sciences, theory of purgatives and emetics). Indeed, according to Rosenstein, albumen appears in the urine from the beginning of the sickness, in adults as well as in children.

Influence of cathartics and emetics. According to my theory on the subject, quite a number of cathartics and emetics produce this action by diminishing more or less rapidly the formation of blood. Hence it would be quite natural for drastic cathartics to give rise to albuminuria; and this has often been observed. Nitrate of potassium itself is liable to render the urine albuminous, whether it is taken in repeated large doses, or in small doses for a long time.

Influence of anæsthetics. According to the same theory, anæsthetics protect from putrefaction after death, and retard blood formation during life. And, in accordance with what was already known of chloroform and several ethers, it is now ascertained that chloral hydrate is anti-putrid after death; that it retards greatly vital combustion; that in repeated large doses it causes nausea and vomiting; that it brings on a relaxation of the tissues; that long taken internally, it may bring on albuminuria, anasarca, and a sort of poisoning resembling that caused by ergot. (Smith, *Boston Med. and Surg. Journal*).

Coffee is antiseptic, sedative, not very energetic; and according to my views, may, when taken in large doses, give a darker color to the blood, diminish the exhalation of carbon dioxide and the production of urea; it may act as a diuretic, and let albumen escape with the urine. The same is true of fuchsine, especially arsenical fuchsine; it is antiseptic, retards hæmatosis, and may be accompanied by albuminuria.

Metallic compounds. Those salts of the metals proper which do not absorb oxygen are generally antiseptic after death, and poisonous during life. In my view, the subsequent death, and the symptoms which precede it, manifest in various degrees an arrest of hæmatosis. All of them may doubtless cause albuminuria; at least, the number of them in which the fact has been ascertained has considerably increased since I first brought out my theory. Then my only observations referred to mercurials,

but now the following are said to act thus: the soluble compounds of antimony and lead (Ollivier); some silver compounds (Lionville); salts of cadmium; nitrate of uranium; various copper salts; arsenical compounds; gold and palladium chlorides.

Moral influences. It is a well-known fact that sad moral affections may diminish hæmotosis to quite an extent. It is also a well-known fact that these may cause diuresis, intestinal evacuations, vomiting, albuminuria (Prout, etc.)

Influence of water taken internally. Upon injecting a certain amount of water into the veins, the blood becomes very thin, there is a marked decrease in hæmotosis, with a sedative effect very marked. As in the preceding cases, the marked arrest of function is accompanied by an escape of albumen into the urine (Magendie). These results have been verified by Hermann.

Influence of oxydizing agents—that is, agents which are known to diminish the quantity of free oxygen, and to rarefy the atmosphere. In phosphorus poisoning, an energetic oxydizing agent, the quantity of oxygen in the blood is markedly diminished. This diminution is otherwise manifested by a marked change in color, by a striking increase of fat, enlargement of the liver and vomiting. What becomes of the albumen of the blood under such circumstances? It passes away with the urine (C. Schultzen).

According to the experiments of Cl. Bernard, *carbon protoxide gas* forms with hæmoglobine a kind of compound more intimate than that resulting from oxygen, so that when it is respired, it has a tendency to displace this gas from the blood globules, and consequently diminish hæmotosis; the temperature is lowered, the amount of urea diminished, while that of uric acid is increased (Ritter). This also justifies my theory, for carbon protoxyde is antiseptic and preserves dead bodies, its inspiration also causes albuminuria. At high altitudes, a rarefaction of the air ought to diminish hæmotosis. Indeed, the blood turns darker, the liver often becomes congested, vomiting, as above described, becomes manifest if the transition is sudden, and urea is diminished in the urine (Bert). Also, albuminuria is much more frequent on high mountains than at the level of the sea (Jourdanet).

The preceding facts demonstrate in the highest degree the

exactness of our theory, and their publication, we hope, will stimulate also to greater progress in that line.—*Revue Medical*, July 30, 1881.

LISTERISM TOTTERING.—Just as every body thought the carbolic spray of Lister had become an established method in surgery, it receives a stunning blow from one of its former advocates, Mr. Keith, the great ovariologist. At the International Congress, according to a letter in the *Boston Medical Journal*, Prof. Keith declared, the subject being under discussion, that he had abandoned the antiseptic treatment altogether. “True,” he said, “I had eighty successive recoveries under Lister’s method, and *stopping there* it would be a wonderful showing. *But out of the next twenty-five I lost seven.* One died of acute septicæmia, in spite of the most thorough antiseptic precaution; three of unquestionable carbolic acid poisoning; one of renal hæmorrhage.” He went on to say that out of the eighty consecutive cases (or rather he said it first) many came too near dying; that a large number got a high temperature—105°, 106°, 107° Fahrenheit—the evening following the operation, but he said “they happened to pull through.” He then said that since he had for four months past abandoned the antiseptic method, and relied upon perfect cleanliness, care in controlling hæmorrhage, and thorough drainage, his cases were giving him much less trouble, and he was getting more satisfactory results.

He now stopped for a few moments, hesitating, as he must have realized the importance of his words, knowing that the whole world—surgical—was lending a “listening ear” to his utterance. The silence was “audible.” Then he raised his head, and looking his audience squarely in the face, he said: “Gentlemen, I have felt it my duty to make these statements, for *they are true*,” and took his seat. I shall not attempt to describe the applause nor the effect of his statements. Prof. Keith, by the way, told me privately that he almost died himself from using the carbolic acid so much. He got renal hæmorrhage and debility to an alarming degree.—*Pacific Med. and Surg. Journal*.

Selections.

THE CASE OF PRESIDENT GARFIELD. By FRANK H. HAMILTON, A.M. M.D., LL.D.

Relative positions of the President and the Assassin—Point of entrance of Ball—Course of the Ball—Location of the Ball—Symptoms immediately following the Shooting—Supposed Course of the Ball—Symptoms pointing to right Iliac Fossa as the location of the Ball—Formation of Pouch of Pus in, and Ossification of Main Channel—Incision below twelfth Rib—Irritability of Stomach—Parotitis—Bronchitis and Broncho-Pneumonia—Atmospheric Influences—Removal to Long Branch—Death—Official Report of Autopsy—Additional Facts in relation to Autopsy—The Impracticability of determining the Course of and extracting the Ball—General Principles established in Gun-Shot Wounds of Belly—Application of Lister's Dressings to the President's Wounds—Impropriety of making a Counter-opening in the long Sinus.

In answer to our inquiries, Dr. Hamilton dictated as follows :

So far as I am informed, the testimony is conflicting as to the relative positions of the President and the assassin when the pistol was fired. It is now rendered probable that the assassin stood well to the right and slightly in the rear of the President. The ball entered about four inches to the right of the spine, penetrating and comminuting the eleventh rib, entering the intervertebral substance between the last dorsal and first lumbar vertebræ, and passing obliquely forward, emerged at a point near the center of the first lumbar vertebra in front ; and was found some distance to the left of the vertebra at the lower margin of the pancreas—being situated nearer its posterior than its anterior surface—wholly without the peritoneal cavity. It is unnecessary to say that the course of the ball, after penetrating the rib, was not determined until after death. I saw the patient on the morning of July 4, in consultation. We were then informed

of the manner of the accident, and that on the receipt of the injury the President had fallen to the floor, sinking down to the right side; that, being interrogated, he complained of pain in his right ankle, and subsequently, in the course of the day, of a similar pain in his left ankle; which pains had been promptly relieved by the hypodermic injection of morphine. He vomited immediately after the receipt of the injury, and in the course of the day his urine had to be drawn once by the catheter. There was not, when first seen by myself, nor has there been at any time subsequently, any apparent loss of power in his lower extremities, or diminution of the natural sensibility at any point. The pains in his ankles, however, were accompanied with hyperæsthæsia of the integument; and a few days later it was observed that there was hyperæsthesia of the integument of the right side of the scrotum. All of these symptoms—the pain and the hyperæsthesia—disappeared wholly in the course of the first week or two, and never returned. On the morning of the fourth of July, the patient, being partially under the influence of the morphine, was not suffering pain, the bowels were tympanitic, and the pulse was feeble. At the first consultation, the question having arisen as to the probable course of the ball, it was stated that Surgeon-General Wales, of the Navy, had on the day of the receipt of the injury introduced his finger to its full extent, and that he had declared that it penetrated the substance of the liver, the structure of which he recognized by its granular feel; and Dr. Bliss stated that he had introduced a probe about three inches, which seemed to have passed in the same direction. This testimony was regarded sufficient to determine that the ball was at least beyond our reach, and beyond the reach of safe exploration. Dr. Woodward had introduced his finger sufficiently deep into the wound to determine that the rib was broken. Finding upon personal examination and inspection that the track of the wound was completely closed by a firm clot, I refused to make any further exploration. From this time forward great uncertainty existed in the minds of the medical attendants as to the actual course and present situation of the ball. On the 24th of July, and after the complete subsidence of the tympanites, a circumscribed point of induration was discovered in the right

iliac fossa, which at once led to a suspicion that the ball had been deflected, coursing along the anterior surface of the lumbar muscles, and that this induration indicated its present seat. This suspicion was sustained by the hyperæsthesia of the right side of the scrotum, which, as Prof. Weisse had already shown in his anatomical observations, would be the natural result of an injury of the ileo-inguinal or ileo-hypogastric nerves, which lie in the course of the then supposed track of the ball. Still further confirmation was added when, on the 27th of July, we found that a flexible catheter could be carried downward in the direction of the supposed situation of the ball to a distance of seven inches. The point of induration in the right iliac fossa gradually moved downward and became more hard and defined, conveying the impression that it was the ball and that it was encysted. At the autopsy, it having been determined that this was not the ball, further examination of the channel in this direction was not prosecuted. Indeed, this induration had entirely disappeared after death, and it is now presumed that it only indicated the lower end of the long sinus already described.

About this period a small pouch of pus was formed in connection with the main channel, extending underneath the integuments of the back, causing rigors, which were at once relieved by a free incision; and a little later rigors followed in consequence of the temporary obstruction of the channel caused by the floating of a small fragment of the rib into the orifice, which was relieved on the removal of the fragment.

On the 8th of August, great difficulty having been experienced in the introduction of the drainage tube into this long suppurating canal, an incision was made below the twelfth rib, the patient being under the influence of ether. About a week later, the stomach of the President became exceedingly irritable, and it was found necessary to suspend alimentation by the mouth, and for three or four days he was nourished only by enemata. On the fourth day after the suspension of alimentation by the mouth the right parotid gland began to enlarge (Aug. 17), and on Aug. 24 suppurated and was incised; the first incision giving exit only to a few drops of pus. Subsequently it opened into the mouth and meatus auditorius externus, and three or four incisions were

made at different points on the surface for the exit of matter. At the time of death the suppuration and swelling of the parotid gland had almost entirely disappeared.

Following the parotitis there was a gradual development of bronchitis in the right lung; and finally a broncho pneumonia of the lower portion of the right lung, indicated by well defined dullness and a total absence of the respiratory murmur in that region. From this time until the period of his removal from Washington there are no events of striking interest worthy of being related in this brief summary, except the alarming weakness and great somnolency of the patient, which occurred on the 24th, 25th and 26th of August, and which led to an apprehension that a fatal issue was at hand. The patient was evidently suffering from atmospheric influences, the heat being intense and oppressive, and most of the time the air being motionless, so that not a leaf could be seen to stir upon the trees surrounding the White House. There was no evidence, however, at any time, that the patient suffered from malaria having its source in the house drainage or the marshes in the vicinity, and which latter at a later time in the season had always been regarded as pestiferous. His removal to Long Branch occurred on the 6th of September, and was effected without injury or discomfort to the patient, with only a slight amount of fatigue, manifested after his arrival, and from which on the following morning he had completely recovered. There was no day while he lay in the cottage at Long Branch that he did not express himself as pleased and even delighted with the change; nor was he ever oppressed by the heat, although one of the days, the first after his arrival, was the hottest day of the season. At two o'clock in the afternoon of this day, when the heat was greatest, in reply to an inquiry, he said he experienced no discomfort. From this time until the period of his death, which was sudden and unexpected, although in no sense unanticipated, there is no incident worthy of special note—except that there was a gradual change in the last two or three days for the worse. The manner of his death and the result of the subsequent autopsy are sufficiently explained in the official bulletin.

[We here insert the official bulletin.—ED.]

“ A post-mortem examination of the body of President Garfield was made eighteen hours after death, in the presence and with the assistance of Drs. Hamilton, Agnew, Bliss, Barnes, Woodward, Reyburn, Andrew H. Smith, of Elberon, and Acting Assistant Surgeon D. S. Lamb, of the Army Medical Museum, Washington. The operation was performed by Dr. Lamb. It was found that the ball, after fracturing the right eleventh rib, had passed through the spinal column in front of the spinal canal, fracturing the body of the first lumbar vertebra, driving a number of small fragments of bone into the adjacent soft parts, and lodging below the pancreas, about two inches and a half to the left of the spine and behind the peritoneum, where it had become completely encysted. The immediate cause of death was secondary hæmorrhage from one of the mesenteric arteries adjoining the track of the ball, the blood rupturing the peritoneum, and nearly a pint escaping into the abdominal cavity. This hæmorrhage is believed to have been the cause of the severe pain in the lower part of the chest complained of just before death. An abscess cavity, six inches by four in dimension, was found in the vicinity of the gall bladder, between the liver and the transverse colon, which were strongly adherent. It did not involve the substance of the liver, and no communication was found between it and the wound. A long suppurating channel extended from the external wound, between the loin muscles and the right kidney, almost to the right groin. This channel, now known to be due to the burrowing of pus from the wound, was supposed during life to have been the track of the ball. On an examination of the organs of the chest evidences of severe bronchitis were found on both sides, with broncho-pneumonia of the lower portions of the right lung, and, though to a much less extent, of the left. The lungs contained no abscesses, and the heart no clots. The liver was enlarged and fatty, but free from abscesses. Nor were any found in any other organ except the left kidney, which contained near its surface a small abscess, about one-third of an inch in diameter. In reviewing the history of the case in connection with the autopsy, it is quite evident that the different suppurating surfaces, and especially the fractured, spongy tissue

of the vertebra, furnish a sufficient explanation of the septic condition which existed.

D. W. BLISS,
J. K. BARNES,
J. J. WOODWARD,
ROBERT REYBURN,
FRANK H. HAMILTON,
D. HAYES AGNEW,
ANDREW H. SMITH,
D. B. LAMB."']

It may be necessary, however, to repeat, inasmuch as contrary statements have been made, that the lungs contained not even the most minute abscess, and that there was no metastatic abscess found in any of the structures examined, except one less than a half inch in diameter near the surface of the left kidney. There were three small serous cysts under the peritoneal covering on the convex edge of the right kidney, each about the size of the vertical section of a large pea. The abscess found between the transverse colon and the liver was evidently not metastatic, but probably was caused by the original injury. There was no cicatrix or wound of the liver, nor anything to indicate that it had suffered injury in the slightest degree.

Since it has been thought by some that it was the duty of the surgeons to have ascertained positively the course and location of the ball, it is proper to consider whether either the one or the other were practicable.

As to determining the course of the ball by a probe, every anatomist will see that it was impossible—if he will consider the very tortuous course which the ball must have taken to reach its final destination; that it passed through the solid structure of the vertebra, and that no metallic instrument sufficiently firm to give indications of the course and direction which it took within the body could ever have reached the ball; nor would any surgeon of experience, familiar with gun-shot wounds in the belly, in the absence of any satisfactory or conclusive evidence as to what course the ball had taken, venture to introduce a probe into the abdominal cavity for the purpose of exploring the supposed track; nor, indeed, if he had evidence as to the course and situ-

ation of the ball, could he have been justified in such an exploration. No point is better settled in surgery than that interference of this sort in gun-shot wounds of the belly is meddlesome, useless and dangerous; and had it been done, and a fatal peritonitis in consequence been set up, the surgeon doing it would have been justly held responsible for the fatal result.

As to the possibility of the extraction of the ball safely, it would have required a large tegumentary and muscular incision as a means of approach to the spinal column; the actual removal of the whole of the twelfth lumbar vertebra in order to furnish a sufficient channel through which the bold surgeon should advance with his instrument for extraction; and after emerging from the cavity thus made in the spinal column, he would have to penetrate or grope his way cautiously between the ganglionic system of nerves, and arteries, veins, lymphatics, including the thoracic duct, all of which are vital structures almost inextricably joined to each other on the front and sides of the spinal column, and the lesion of any one of which must have proved inevitably fatal.

Throughout the whole course of the treatment, contrary to what has been publicly said repeatedly, so far as it was possible to apply the system of antiseptic surgery advocated by Mr. Lister to a wound of this character, it was rigorously employed.

I am reminded now to say, in reply to some suggestions made from time to time, that we ought to have made a counter-opening in the lower portion of the long sinus which terminated in the right iliac fossa; that there was no period of time during the progress of the case in which we felt absolutely certain that what we recognized in the fossa as a point of induration was the ball; nor were we entirely certain at any time where the lower end of the sinus was actually situated; nothing but a very flexible instrument could ever be introduced, and inasmuch as when introduced, its presence in the track could not be recognized by the sense of touch, we were left without any means of determining, with a sufficient degree of accuracy to justify an operation, where the lower end of the channel was. Indeed, it is probable that the flexible catheter employed never reached the lower end of the channel, but doubled upon itself near the crest of the ileum. To have cut through, or between, the great mass of muscles in

the lower portions of the lumbar regions, for the purpose of making a counter incision into a small channel, the course of which we did not and could not know, even approximately, would have been, under any circumstances, an unjustifiable procedure—and especially so in the case of the President, whose hold upon life during all this long period seemed to depend upon a thread.

ON THE TREATMENT OF THE VARIOUS FORMS OF CONSUMPTION.

BY ROBERTS BARTHLOW, M.D., LL.D., Professor of Materia Medica and Therapeutics in the Jefferson Medical College, Philadelphia.

I will take it for granted, as is generally believed, that there are three distinct forms of pulmonary consumption: Chronic, catarrhal, or caseous pneumonia, which is an essentially inflammatory disease; chronic tuberculosis, with special direction to the pulmonary parenchyma, which is a diathetic malady, and fibroid phthisis, which is a chronic interstitial pneumonia ingrafted on a chronic bronchitis. It must be obvious that if this classification has a proper basis, it does not suffice to propose a plan of treatment for consumption, but our therapeutical methods must be guided and controlled by the pathological conditions. Whilst this is true, there are certain general principles of treatment applicable to all forms. I propose to consider these first—they are climate and personal hygiene.

In considering the subject of a suitable climate for a pulmonary invalid, I will not go beyond the limits of the United States, within which are contained the utmost variety, and, indeed, the perfection of health resorts for this purpose. In the absence of any statistical data showing the results of prolonged residence in particular localities which might indeed settle the question, we have some general principles to guide us, too little regarded by the profession, but of great value. We owe to Dr. Bowditch, of Boston, the eminent physician and sanitarian, the first principle, which he established for Massachusetts, and which has been confirmed on a larger scale for England. The Bowditch generalization is, that there is a constant ratio between the number of cases

of consumption and the amount of water—rainfall, and collections of water in streams, ponds, and lakes. This principle is not applicable to the ocean, where other conditions obtain. Certain parts of England, having had a large mortality from consumption, present a very different report since suitable drainage works have been put into operation. You need only to cast your eye over the elaborate Atlas of Medical Geography, by Lombard, to see how large a part excess of moisture plays in the geographical distribution of phthisis. All along the sea-coast are traced the deeply shaded lines, whilst in the elevated interior regions, the mortality has disappeared. It is true that density of population and other evil hygienic influences are at work, but excess of moisture is a large factor.

Next to dryness of soil and climate as a remedy for consumption is elevation. The fact stands out as conspicuously in the great Atlas of Medical Geography as the previously considered influence. In the elevated regions—the great plains and plateaus of India, Africa, and America—phthisis is almost unknown (Lombard, vol. iv., p. 420.) Elevation has an important influence in the relief of phthisis, because the air is dry and rarefied. Breathing rarefied air lessens intrathoracic pressure, increases the rate of the respiratory movements and the rapidity of the pulmonary circulation. Residence in a rarefied atmosphere increases the rapidity of the circulation and the amount of blood in the peripheral vessels. The influences of these factors in promoting digestion, assimilation, and tissue metamorphosis are unquestionable. Uniformity is only less important as a requisite for a climate for pulmonary invalids. The reason of this requirement is obvious. Those invalids in a condition to be benefited by outdoor exercise need an equable temperature in which to pursue their sports or recreations with safety. But more important than this is the bad influence of a variable climate in causing attacks of bronchial catarrh, a morbid process so much concerned in the production of caseous phthisis. Applying these principles to the question of a climate for consumptives, I place first on the list the great plains and plateaus of our interior continent, next certain parts of California, then a limited district, of which Aiken, South Carolina, may be regarded as the center, and lastly, the

upper lakes and Minnesota, and the Red River of the North. In the limited space of such a lecture it is quite impossible to go further into details on the subject of climate.

Amongst the questions of personal hygiene alimentation, is the most important. And here, gentlemen, I have some important facts to submit to your consideration. We owe to Hughes Bennett the important practical suggestion that the initial disorders of phthisis are stomachal—that before any pulmonary trouble arises there is a period during which the organs of digestion do poor work, prepare the aliment imperfectly, and hence the nutrition of the body declines. That acute minded physiological surgeon, Mr. Jonathan Hutchinson, has investigated this, as so many other subjects, in a quite original manner, and he finds that almost universally a form of dyspepsia precedes phthisis. In fact it is a matter of common observation, that the individual is out of condition, is losing ground, and this long before the manifestations of pulmonary lesions of any kind. A chronic gastro-intestinal catarrh is present, and one of its special features is that the patient has a positive distaste for the fatty constituents of the food, and that the power to digest fats is much impaired. In a disease characterized by failure of nutrition, and in which the process of repair is so feeble, it is of the utmost importance to maintain the digestion and assimilation at the highest efficiency. There is a general agreement on this point, and hence no sooner is the diagnosis of phthisis arrived at than the injunction of active feeding follows. Without any attempt at correcting the morbid state of the digestive organs, they are plied with the richest hydrocarbons, with a teaspoonful of cod-liver oil before each meal, by way of appetizer, and a tablespoonful or more of whisky after meals as a digester. It results from this method that the appetite is utterly destroyed, and the food, swallowed with disgust, is either rejected or remains to distress. I hope you will always bear in mind that it is not the quantity of food swallowed, but the amount digested and assimilated which is important to the welfare of the individual. The diet, then, should be carefully regulated to the requirements of the individual. Let me now give you a practical hint, on which you will be able to act with great good to your patients. *A period of rest to the*

stomach by a minimum diet will enable the organ to recover from the morbid state, and will induce a great appetite. Acting on this rule, I have repeatedly effected great improvement in the condition of phthisical subjects at the initial period. Put the patient on milk for a week or two, and then gradually construct a suitable dietary. To remove the condition of gastro-intestinal catarrh there are several remedies on which we may rely with confidence. Arsenic stands first, probably, and is to be given in small doses—two drops—three times a day, before meals. Scarcely inferior to arsenic is the combination of iodine and carbolic acid: $\mathcal{R}$. Tinct. iodinii. $\mathfrak{z}$ j. acid. carbol. $\mathfrak{z}$ ss. M. Sig. One or two drops in a tablespoonful of water three times a day, before meals. The oxide and nitrate of silver are also highly serviceable, but unfortunately they cannot be continued long, owing to the danger of argyria. The mineral acids, if the digestion merely flags, or if the previous remedies have been taken for a sufficient time, are very important remedies. I have seen some striking results from the use of the nitro-muriatic acid during the initial stage, but the action of this remedy in changing morbid states of the mucous membrane and in promoting appetite and digestion is the secret of any curative power it possesses. The alkaloid strychnia, dissolved in diluted muriatic acid, makes a useful combination, for strychnia is probably the most effective agent which we possess for arresting the vomiting of phthisis.

If the morbid state of the mucous membrane has been removed, and the appetite restored, then we may consider the question of forcing the nutrition by increasing the performance of the assimilative organs. Now we may give cod-liver oil, and aid its digestion by the administration of malt liquors or alcohol in some form: It should be given after meals, to be digested and assimilated with the other foods. It is a great error to administer the oil, as is so often done, on an empty stomach and some time before the meal, because it will spoil the appetite, and keep the stomach at work during its proper period of repose. Furthermore, the quantity of oil taken should rarely exceed a teaspoonful, for this amount only can the digestive organ properly assimilate at one time. If it impair the appetite and cannot be digested, it is useless, and should not be continued. Bernard has put us in posses-

sion of an important fact, which ought always to be utilized—that is, that ether promotes the digestion of the oil when administered with it. Bernard's notion was that ether increases the flow of the pancreatic juice, and must therefore promote the emulsionizing of the oil. Whether or no the theory be correct, there has been accumulated sufficient testimony to show that the digestion of the oil is much more easily and perfectly accomplished by the addition of some minims of ether.

To what extent is the administration of alcohol desirable and proper in the treatment of phthisis? First of all, we should take our position firmly against a too prevalent notion that whisky is in a certain degree antidotal, and that any case of consumption must be cured if only it is given early enough, and in sufficient quantity. The mischievous fallacy is so far from true, that we now know a form of degeneration of the pulmonary parenchyma is produced by alcoholic excess. Before hectic comes on, as a rule, the extract of malt and malt liquors are preferable to whisky and brandy. When wasting proceeds rapidly, and destruction of the lung tissue is going on at the same pace, when the fever approaches more and more the septicæmic type, the stronger liquors are better, but at any period whisky should be given instead of malt liquors, if it agrees better. Is there any guide to the proper administration of alcohol? I am perfectly clear that these are well-defined principles. That quantity of spirit which increases the appetite, and improves the digestion, is the proper quantity. An excess is hurtful, because the alcohol precipitates the pepsin from its solution in the gastric juice, and therefore suspends the production of peptones. A large quantity taken on an empty stomach is without influence on the digestion of foods, and whilst it affects the mucous membrane injuriously, after absorption acts on the hepatic cells. These reasons seem conclusive in favor of a moderate quantity of alcoholic food—say half an ounce of whisky—taken after each meal.

[CONCLUDED IN JANUARY NUMBER.]

Items.

TUBERCLE: ITS HISTOLOGICAL CHARACTERS, AND ITS RELATION TO THE INFLAMMATORY PROCESS, AS SHOWN IN "TUBERCULOSIS" OF LYMPHATIC GLANDS. By FREDERICK TREVES, F.R.C.S. Read before the International Medical Congress.

The term was here limited to the appearance known as sub-miliary, lymphoid, or primitive tubercle, or tubercular follicle. Tubercle, the author said, represented a certain stage or degree of a peculiar form of inflammation; that developed tubercle was usually chronic, and induced by slight excitation; its exudations were very cellular, and in certain places presented large nuclear-like bodies (Rindfleisch), which might be regarded as almost special to the process; the products resisted absorption, and lingered in the tissues. Non-vascularity of the part was an early feature. Degenerative changes usually ensued, most commonly caseation. If a certain stage of the process were reached, giant-cells appeared, or the so-called tubercle was met with. Above all, this inflammation required for its development the anatomical element of adenoid, or lymphatic tissue. In the tubercle producing process in glands, the first changes are those of simple inflammation. The appearance of giant-cells, or of tubercle proper, indicated a certain stage in the process. The stage might never be reached, and glands, if affected with some intensity, might caseate before any giant-cell or tubercle had been seen. Tubercle occurred in the most chronic or least intense form of the inflammatory process. It represented, in one sense, the highest attainable morbid change. Giant-cells were by no means special to tubercle. They were merely lymph-coagula, and indicated the

cessation of all lymph current. In regard to histology, Mr. Treves said that the fibrous material of glands, the seats of tardy inflammation, tended to arrange itself in roundish districts. If the fibrous matter in these districts were somewhat open, and giant-cells were introduced, the appearance of tubercle might be seen. The reticulum occupying the spot presented no constant appearance, but was modeled precisely upon the arrangement of the fibrous matter in the vicinity. Many of the so-called tubercles had the giant-cell at the periphery, others had no giant-cell, many are oval, or of the most irregular outline. The giant-cell, he urged, was here also merely a lymph-coagulum, that blotted out a certain amount of the reticulum in which it was deposited. As the mass degenerated, the reticulum often came into view, and could be seen to be continuous with the adjacent tissue by means of the giant-cell processes.

OFFICIAL LIST OF CHANGE OF STATIONS AND DUTIES OF MEDICAL OFFICERS of the United States Marine Hospital Service, July 1, 1881, to September 30, 1881:

Bailhache, P. H., Surgeon. Detailed as member of Board to examine keepers and crews of Life Saving Stations. September 8, 1881. To inspect the service of stations in Maine, New Hampshire and Massachusetts. September 9, 1881.

Wyman, Walter, Surgeon. When relieved of special duty as medical officer revenue-bark "Chase," to rejoin his station, *via* Washington, D. C. August 18, 1881.

Long, W. H., Surgeon. Detailed as Chairman Board of Examiners. September 12, 1881. Granted leave of absence for twenty-four days from September 24, 1881.

Purviance, George, Surgeon. Detailed as member Board of Examiners. September 12, 1881.

Sawtelle, H. W., Surgeon. Detailed as recorder Board of Examiners. September 12, 1881.

Godfrey, John, Passed Assistant Surgeon. To proceed to Pascagoula, Miss., as inspector. July 27, 1881.

Goldsborough, C. B., Passed Assistant Surgeon. To proceed to Havre de Grace, Md., as inspector. July 27, 1881. Granted leave of absence for thirty days from September 1, 1881.

Cooke, M. P., Assistant Surgeon. Granted leave of absence for thirty days from August 12, 1881.

Carter, H. R., Assistant Surgeon. Granted leave of absence for eight days from September 24, 1881.

SOCIETY MEETINGS.

Chicago Medical Society—Mondays, Dec. 5-19.

West Chicago Medical Society—Mondays, Dec. 12-26.

Biological Society—Wednesday, Dec. 7.

CLINICS.

MONDAY.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Prof. Holmes; 3 p. m., Otological, by Prof. Jones.

Mercy Hospital—2 p. m., Surgical, by Prof. Andrews.

Woman's Medical College—2 p. m., Dermatological and Venereal, by Prof. Maynard; 3 p. m., Diseases of the Chest, Prof. Ingals.

TUESDAY.

Rush Medical College—3 p. m., Dermatological and Venereal, by Prof. Hyde.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Quine.

WEDNESDAY.

Chicago Medical College—2 p. m., Eye and Ear, by Prof. Jones.

Rush Medical College—2 p. m., Medical, by Dr. Bridge; 3 p. m., Ophthalmological and Otological, by Prof. Holmes; 3:30 to 4:30 p. m., Diseases of the Chest, by Dr. E. Fletcher Ingals.

THURSDAY.

Chicago Medical College—2 p. m., Gynæcological, by Prof. Jenks.

Rush Medical College—2 p. m., Diseases of Children, by Dr. Knox; 3 p. m., Diseases of the Nervous System, by Prof. Lyman.

Eye and Ear Infirmary—2 p. m., Ophthalmological, by Dr. Hotz.

Woman's Medical College—3 p. m., Surgical, by Prof. Owens.

FRIDAY.

Cook County Hospital—2 to 4 p. m., Medical and Surgical Clinics.

Mercy Hospital—2 p. m., Medical, by Prof. Davis.

SATURDAY.

Rush Medical College—2 p. m., Surgical, by Prof. Gunn; 3 p. m., Orthopædic, by Prof. Owens.

Chicago Medical College—2 p. m., Surgical, by Prof. Isham; 3 p. m., Neurological, by Prof. Jewell.

Woman's Medical College—11 a. m., Ophthalmological, by Prof. Montgomery; 2 p. m., Gynæcological, by Prof. Fitch.

Daily Clinics, from 2 to 4 p. m., at the Central Free Dispensary, and at the South Side Dispensary.

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